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SOCIAL THEORY AND SOCIAL ACTION[1]

F. STUART CHAPIN
University of Minnesota

WHAT HAS social theory to say about social action? Since men do not act on biological motivations of hunger, fear and sex alone, but are guided by their underlying philosophy of economic, political and social relations, it is worth while to inquire into the connections between contemporary social theory and contemporary social action.

Clarification of thought about the complexities of contemporary social action may be promoted by the simplifying device of a two-fold division. We may mark off two different but over-lapping fields of social action. The first consists of planned social action directed toward goals. This is the area of decisive leadership, of legalistic political action in law-making and law enforcement, and of impulsive social reform movements. These forms of social action are interrelated and from them emerge the more personal social forces. The second field of social action consists of the unintended consequences that follow from the interrelationships among the personal social forces. Let us consider a few illustrations of unplanned consequences.

The unsettling effects upon the silver currencies of China and Mexico of the present silver purchase policy of the United States Government is an unplanned result of the relations between planned actions set up by the silver bloc in Congress and the plans of those

[1] Presidential address, Thirtieth Annual Meeting, American Sociological Society, New York City, December 30, 1935. The papers following this address in this number of the *Review* were all read at the same Annual Meeting, December 27–30, 1935.

who believe in a special monetary theory. In this field we observe the unplanned results of the interrelations of independently planned social actions. Thus unplanned combinations of blocs of interrelated human behaviors set up impersonal social forces not intended by the authors of particular plans. But unplanned results flow also from the combinations of individual acts. Thus we observe that a run on a bank leads to its closing and this to the failure of a creditor of the bank. We see the phenomena of hoarding contribute to a government's decision to go off gold.

These activities of hoarding or dishoarding of money are all the results of innumerable individual decisions. But since these decisions occur in a society equipped with a marvelously efficient network of communication agencies, the individual acts do not remain independent. They are cumulated, combined and transmitted with ever-increasing potential to remote areas, and there they may lead to results that are unplanned. We note that the crowd psychology of a panic undermines the basis of essentially sound business enterprises located in areas distant from the crisis.

All these cases illustrate how impersonal social forces may be set in motion by the unplanned combinations of independent individual behaviors. We call these phenomena impersonal social forces because the chain of cause and effect may be so long and complicated that the consequences cannot be interpreted as the realization of plans of particular persons. Furthermore, this type of phenomenon seems to be both more extended and more intense today than ever before. The reason appears to be found in the channels of transmission which are provided by modern mechanical agencies of communication and transportation. For centuries the interdependence of different regions and different nations was expressed in the structure of slow-moving foreign commerce. Today the telegraph, telephone, radio, newspaper, motion picture, airplane, automobile, railway and steamship line radiate information in a flash or move articles with speed. Consequently the formerly stable structure of social interdependence is subject to rude shocks and sudden pressures. This comes about because of the marvelously efficient network over which information and goods may be collected from widely separated sources and then move with increasing force through channels that converge on important centers.

Let us consider an example of planned and unplanned social action in combination. The Social Security Act represents a compromise be-

tween many forces each driving toward ends that different leaders regarded as socially-desirable goals. Experts on insurance, actuaries, economists, labor leaders and social workers, all led different factions who disagreed about the means to attain the goal of social security for the masses. Then there was the pressure of the Townsend old-age pension movement. This impulsive social reform gathered unto itself the spontaneous but unexpressed wish fantasies of that bewildered generation, the contemporary aged. Its panacea spread like wild-fire through the dry grass of hopeless yearning for security. Modern communication agencies facilitated its organization into a social movement. Congress and the executive were deluged with petitions. Political considerations of different sorts interfered with the Simon-pure expression of the plans of special groups and the Social Security Act came as the most practicable compromise of conflicting views. But out of the patch-work combination there may emerge certain unplanned results. The consequences of investing the huge reserves to be accumulated under the procedures of Federal Old-Age Benefits seem not to have been fully considered by those who were responsible for the final formulation. If the reserves[2] are to be invested in government bonds purchased during a boom period and hence stimulating to the investment market, what of the depreciating effects from the sale of bonds during a depression? Is this sort of investment of reserves not more likely to intensify the liquidation process at just the time when it needs checking?

We may now ask the question: Is there in social theory any counterpart to this rough dichotomy of the field of social action? It seems to me that there is. I would mark off an area to be called *normative* social theory from another area to be called *non-normative* social theory. Actually there is some over-lap but the dichotomy is useful because it leads to further distinctions, insights and meanings.

Under normative social theory I would include all utopian ideologies such as evangelical religious systems, the theory of social reform, the theory of state socialism, the theory of the communistic dictatorship of the proletariat, the theory of the coöperative commonwealth, etc. It is characteristic of all of these theories that they are concerned with formulating the principles that *should* guide social action in the use of appropriate means to the attainment of de-

[2] "The investment of unemployment reserves and business stability," Part IV, in A. H. Hansen, M. G. Murray, R. A. Stevenson, and B. M. Stewart, *A Program for Unemployment Insurance and Relief in the United States*, University of Minnesota Press, 1934, especially pp. 184–185, 194–195.

sired ends. In contrast, the non-normative theories include theories of interest, theories of profits, theories of monopoly, theories of business cycles, theories of crowd psychology, theories of the culture lag, theories of state functions, etc. It is a characteristic of these theories that they are concerned with formulating the principles of relationship that seem to explain the consequences that follow from certain sequences of social action. They occasionally try to do more than state how these occur; they sometimes attempt to explain why these occur.

In the area of overlap between the normative and the non-normative theories, there lie theories of sovereignty, theories of social progress, theories of social control, theories of value, etc. All these areas involve opinions.

In normative social theories we encounter value judgments that stress differences in kind. In the non-normative social theories we more frequently encounter quantity judgments that stress differences in degree within each kind. As a result of these differences, normative social theory is able to explain planned social action better than it can explain the unplanned results of the interrelated but independently planned social actions. The reason is that the mental set of the normative theorist is to explain results by treating impersonal consequences as if they were ends or goals. On the other hand, the non-normative social theorists are more successful in explaining why certain unplanned results follow from the interrelations of independently planned actions than they are in formulating the socially acceptable procedures that should be followed to attain a popularly desired goal. The reason is that the mental set of the non-normative social theorist is to abstract impersonal principles from social situations that were essentially personal in origin. This is part of his effort to be objective. But in so doing he may underweigh the role of human motivation, desire, mores, public opinion and value judgments. Out of these differences in mental set there arise the familiar controversies[3] that rage about the topic: What is the proper scientific method in sociology? The generalizing and synthesizing type tends to construct normative social theories and the analytical and investigative type tends to construct non-normative social theories. From the inter-play of criticisms between these two opposing groups of students there arise clarified ideas about the possible relationship of social theory to social action. In attempting to distinguish

[3] Bernard, L. L. "The Great Controversy," *Social Forces*, Vol. 14, No. 1, pp. 64–72.

these two different trends in thought, I have no desire to offer a basis of reconciliation between them. Far be it from me to attempt to resolve a conflict in views which continues to be socially useful, because it forces the two sides to restate and clarify their concepts and theories.

The field of unplanned social action has been greatly extended and intensified by modern technological devices in communication and in transportation. Consider the consequences of conflict among government functions that follow when new government agencies are set up to attain ends described by utopian ideologies. Reflection upon this situation indicates the need of adjusting normative social theory to the facts of our mechanical age, so that we do not continue to plan social action as if single goals could be simply attained without setting up dangerous social and economic reverberations. Here non-normative social theory needs reformulation, so that it can be used to check expansive social action based upon inadequate theoretical assumptions. Too often these assumptions ignore the clear implications for social theory of the new mechanical devices of communication, which make almost too easy the establishment of new social structures. Right here it seems clear to me that the chief implication for social theory is that non-normative explanations must receive more consideration than hitherto, if we are to have a realistic social theory that squares with the expanding field of mass phenomena. The increase of mass phenomena of crowd psychology is greatly facilitated by machine communication used for propaganda; and the mass phenomena of specialization of economic function by social classes is promoted by the use of power machinery for quantity commodity production. Hence we need more widespread study of non-normative social theories. On the other hand, it is true that the expansion of the functions of government, local, state and national, requires that the narrow type of non-normative social theory be accommodated to the facts of human behavior. For, as social action is discharged through the social machinery of legalized political institutions, the role of opinion, desire and emotion, must be reckoned with.

All social theory must rely heavily upon concepts. In the early stages of formulation, purely verbal concepts that describe undisciplined impressions naturally play a major role. But if social theory is to advance beyond the dangers of verbalism of emotion and attain the stability given by objective experience, it is necessary that a

larger and larger proportion of social concepts be operationally defined. That is to say, social concepts should be more and more defined in terms of the operations and processes used in attempts to measure them or to test them by experiment. Now the significance of the operational definition of social concepts is only beginning to be appreciated. There are two reasons for this. First, it was necessary for sufficient time to elapse before the inadequacy of purely speculative social theory could be demonstrated. And this has certainly been done in a most decisive fashion during the present depression. Second, it required time and repeated experiment to construct dependable instruments of social measurement. In fact, this latter development is very slow to come, due to the absence of basic scientific training of young students in the social sciences, and due to the actual opposition of social scientists with interests that were primarily philosophical or normative. Nevertheless, there are a few indications of a beginning at the operational definition of social concepts. Examples of this development are the studies of public opinion by S. A. Rice and M. M. Willey, the more recent scales to measure social attitudes invented by C. Kirkpatrick and R. F. Sletto, the mores scales of C. C. Peters, the social status scales of Mary J. McCormick, F. S. Chapin and Alice Leahy, the studies in the definition of social contact or group contact by Dorothy Thomas and her associates, and the experimental studies of S. C. Dodd. In these we find illustrations of the operational definition of such social concepts as public opinion, social attitudes, mores, social status, social contact, and social experiment. So far as these concepts are involved in the formulation of social theory, operational definitions of them are now at hand.

The operational definition of the concepts of normative social theory, such as social class, wage slaves, proletariat, bourgeoise, socialite, aristocrat, plutocrat, etc., is difficult. On the other hand, the concepts of non-normative social theory, such as income velocity of money, transaction velocity of money, social change, invention, diffusion, social attitudes, social status and mores, are not so difficult to define operationally.

Since the mores are among the most widely used of concepts in social theory, let us examine more closely the operational definition of mores developed by C. C. Peters[4] in his attempt to measure the ways in which and the degree to which motion picture plays violate

[4] C. C. Peters, *Motion Pictures and Standards of Morality*, The Macmillan Co., 1933

the mores and thus perhaps undermine the morals of American youth. Peters performed the following operations in the construction of two of his many scales: one to measure the mores of socially democratic attitudes, and one to measure the mores of parental treatment of children. Starting with the hypothesis that the extent of badness or goodness could be measured "by the degree of shock indicated by the proportion of members of the group aroused to resentment by it,"[5] it was possible to get "these intangible and elusive social phenomena into quantitative form so that they could be subjected to statistical investigation." Thus there were collected from the observation of motion-picture scenes, and other sources, a large number of fragments of conduct. These were carefully written up and classified by pattern to discover gaps and to fill them with new scenes. In this way 82 scenes were found for democratic attitudes and 78 scenes of the treatment of children by parents. Next, these scenes were carefully calibrated, that is, given quantitative values for degree of "badness" after having five different groups of persons sort out the descriptions of movie-scene behavior into three piles. In one pile were those scenes felt intuitively to be more or less wrong, in another pile were those approved or admired, and an intermediate pile consisted of matter-of-course behavior. Thus the extent to which the scenes shocked members of society, and hence violated the mores, was indicated by the proportion of these readers in whom they aroused resentment or admiration. It is evident that when mores are defined in this way they are described in terms of the physical processes of behavior actually performed. Hence, to paraphrase Bridgman, the whole description reduces to an account of an actual physical experience, and must have the same validity as that of all direct observation of physical fact. It relies on sensory responses to clarify abstract thought. Moreover, such concepts, being framed in terms of operations actually performed in physical experience, must lead, at any stage of the research, to "conclusions in which room is left for future refinements within the uncertainties and approximations of our present physical operations."[6]

But before the importance of the operational definition of social concepts can be really understood, it is necessary to resolve a confusion in thought that is present in many discussions of the procedures of experiment and of measurement. The opinion that experiment can

[5] *Ibid.*, p. 8.
[6] P. W. Bridgman in *Scripta Mathematica*, Vol. 2, 1934, p. 1.

not be used in social research is an erroneous judgment. It is wrong because it is based upon a misconception of the real nature of experiment in the physical sciences. Now it is generally agreed that experiment is observation under conditions of control. This being so, the misconception creeps in when control is identified with or limited to the processes of physical manipulation. It is true, of course, that in the physical laboratory the scientist obtains control by manipulating devices that increase or reduce air pressure, or which raise or lower temperature, etc., but the real essence of this process is not the manipulation, it is the reading on the pressure gauge and on the thermometer scale. If the scientist went to the top of Pike's Peak he could find there already existing in nature a lowered air pressure and a lowered temperature identical with the laboratory readings on his pressure gauge and thermometer. As a matter of convenience and efficiency he reproduces artificially in the laboratory the conditions which he could have found in distant places, but the point is that control is attained when the measurements are the same.

In contrast to this advantageous laboratory situation, the argument runs that, because you can not physically manipulate human beings as you do materials in the physical laboratory, therefore you can not have experiment in social research. It is true that you can not produce an I.Q. of 50 in the laboratory by taking a normal person and subjecting him to such a degree of pressure that he becomes an imbecile, or produce a manic-depressive case by heating up a normal person. But this is not the point of the problem. You can go out and discover in society or in institutions individuals whose I.Q.'s measure 50. No social scientist wants to obtain control by physical manipulation of persons. He does not need to do this, for as soon as he has a valid scale to measure intelligence, social status, public opinion, social attitudes, etc., all that he has to do is to get measurements of representative groups of population on each of these scales. Then he *can control* intelligence, social status, social attitudes, etc., for purposes of experiment, *by selecting* a control group and an experimental group whose members have the same distribution of measurement on these scales. Having secured control by the device of selecting those individuals who show the same measurement on different variables, he may then proceed to the study of the way in which two uncontrolled variables are related. Thus while identical measurements indicative of control are obtained in the natural sciences by physical manipulation of the things studied, in social research the

similar measurements that are indicative of control are obtained by selection. In the one case there is *physical manipulation* of materials, in the other case there is *selection* of materials, but in both cases the final test of control is in the identity or *equivalence of measurements*. This being the case it is evident that the possibility of measurement determines the possibility of experiment, and the possibility of both measurement and experiment determines the possibility of getting an operational definition of concepts. Now measurement is merely quantitative description and is especially difficult to develop in the study of social relations. Nevertheless, the scales mentioned illustrate beginnings and as time passes there will no doubt be evidence of additional advances of this sort.

The most thoroughgoing illustration of the experimental method in sociology is the recent report of S. C. Dodd[7] of an experiment in rural hygiene in Syria. After some years of painstaking research in preparation and testing, he developed a scale to measure personal hygiene in terms of the behavior of native Syrians. Two samples were selected for study: an experimental village in which a hygiene program was to be put on; and equated control villages, without such a program. Before the program was put on in any of the villages, all were measured with the hygiene scale. After two years all the villages were again measured. Differences in the second scale position of the two samples was taken as a measure of the effects of the program, other things being as nearly as possible equal in terms of the precautions taken. This really important study is not widely known. Most reviewers of the book display an astonishing ignorance of scientific procedures. In fact, their reviews are little more than so many examples of indecent intellectual exposure.

Before leaving this point, however, we may clarify it a bit further by quoting a recent statement of Murchison. "Perhaps the chief characteristic of experimental science is that its ideas are simply names for certain series of operations that are highly reproducible. It is this quality of reproducibility that sets the operations of experimental science apart from the general operations of nature."[8]

What then is a *true* social theory? I have been careful to phrase the question in this way so that there may be no doubt about the point at issue. As phrased, the question implies that there is a true social

[7] S. C. Dodd, *A Controlled Experiment on Rural Hygiene in Syria*, American Press, Beirut, 1934.

[8] C. Murchison, "Pareto and Experimental Social Psychology," *Journal of Social Philosophy*, Vol. 1, No. 1, Oct. 1935.

theory which is evident in its finality, infallibility, or immutability. The question so phrased is without scientific meaning, because it implies the possibility of absolute formulations to describe and explain relational phenomena. We should, therefore, re-state the question, What are the criteria of a good or a *sound* social theory? Many answers could be made to this question. J. F. Brown[9] has suggested four criteria of a good theory: "(1) It should be economical in that it should be based on the fewest and simplest postulates, which will adequately integrate the experimental data. But economy must not be purchased at the price of neglect of facts; the best theories of today are *not* those most easily understood by sophomores. . . . (2) The best theory should be the only possible theory, i.e., the facts will not be as adequately explained by any other, and any other will contain contradictions. . . . (3) The best theory should be fruitful in the sense of leading to an accumulation of integrated facts. . . . (4) The theory must yield postulates to which universal assent may be obtained. . . ."

The rough dichotomy we have drawn among the patterns of social suggests a somewhat different set of conditions for a sound social theory. We contend, therefore, that sound social theory avoids connotations of any absolutistic categories. Sound social theory needs to be relativistic in the sense of being a statement of probabilities in the situation. It utilizes the tentative formulations of working hypotheses. Nevertheless it does postulate an arbitrarily selected but explicitly stated frame of reference to supply the points of departure for research. It utilizes concepts that are defined as far as possible by the operational method. That is, every social situation is to be described in terms of concepts which themselves are defined in terms of physical processes of experiment and measurement actually performed. Thus the use of the operational method to define concepts avoids the verbalism of emotional attitude, which embraces utopian ideologies that are serving as unconscious mechanisms of escape or of personal identification, and it substitutes for these a verbalism that is descriptive of objective experience. Then mathematical equations are *fitted* to observations and *not* used to *symbolize* a new word which is substituted for another word, which in turn merely ex-

[9] J. F. Brown, *The Mathematical Conceptions Underlying the Theory of Psychological and Social Fields*, Edwards Bros., 1935, pp. 25–26. For an example of the use of mathematical conceptions by implicit rather than explicit procedures and without the forbidding terminology of Brown, see F. Stuart Chapin's *Contemporary American Institutions*, Harper and Bros., 1935, especially pp. 319–352.

presses emotional attitude. A sound theory constitutes a logical system of relations among concepts, postulates and hypotheses, all of which taken singly are so many provisional tools used to interpret experience in a meaningful manner. It scrutinizes the discrepancies between levels of symbols: levels that begin with the more concrete symbolic substitutes for social reality and ascend to the more abstract symbolic substitutes. It examines such systems to insure that the transitions from evel to level are logically made and do not depart from representations of reality to a degree that creates invalidating errors.

In conclusion, how may this sound social theory be applied as a guide to social action? The first application of sound social theory is to examine critically all utopian ideologies that underlie planned social action. The second application is to forecast consequences arising out of the unplanned combinations of those social actions that are independently planned. Hitherto the social scientist trained to do this work with competence has not been able to bring his technical knowledge to bear upon the problem in concrete cases. He has expressed his views in articles of a scholarly sort which probably never come to the attention of the public administrator or the business leader because they are tucked away in journals unknown to the public and couched in technical language unintelligible to most readers. He has been casually consulted, it is true, but in a purely chance contact.

But a change has come. State planning boards now exist as the needed social machinery to implement the function of the technical adviser. All projected legislation designed to achieve social and economic objectives by initiating changes in the existing structure of social organization should be referred to such planning boards for comment. There should be attached to such boards, as technical consultants, persons who are trained social scientists. Such persons should be actually advisory in the sense of being consulted *before* and *not after* administrative decision is made. They should render an expert opinion by stating the alternatives of social action and/or the probable consequences, and submit this statement to the proper public official or leader. Finally, this expert opinion should be considered and studied by administrators before the decision is announced publicly.

IS ACCURATE SOCIAL PLANNING POSSIBLE?[1]

PITIRIM A. SOROKIN
Harvard University

THE VERY title of my paper may evoke a smile: "Don't you know that the Soviet, the Fascist, the Hitlerite, the Rooseveltian and many other governments have been doing social planning and enforcing it? Don't you know that many leaders of business and of various associations have been planning the functions and activities of their respective institutions? Then why beat about the bush with your futile academic 'Is Social Planning Possible?' Since it is practiced, evidently it is possible."

To such a smile I can humbly answer: "Yes, I do know that it is practiced; I know also that in some form or other social planning has been carried on by society since time immemorial; I know that it was practiced by primitive man; undertaken by the governments of many past empires,—Ancient Egypt, Ancient Rome (beginning especially with Diocletian), Ancient Peru and Mexico, certainly Ancient China, and many others; and their planning was on a vast scale; it was even centralized and 'rationalized' in accordance with the science and wisdom of their time. I am aware also that some of these plans are remarkably like, indeed one may say almost identical with, the government-planning of the present time. All this and more I happen to know. If, however, I inquire, 'Is Social Planning Possible?' I mean, in such an inquiry, to be concerned, not with the possibility of social planning of any sort whatsoever, whether wise or foolish, but with the possibility of such planning as that in which the *results achieved coincide with those foreseen, and exactly in the way that was intended.*"

When the question is put in this form, it can hardly evoke a smile. It is a real and hitherto unsolved problem. As an unsolved problem it still needs serious analysis. Such an analysis obviously cannot be made within the space of this short paper. All that I can do for the moment is to lay down some considerations which seem to be rele-

[1] The author is indebted to the Harvard Committee for Research in the Social Sciences for financial help in preparation of this study.

vant to the problem and may contribute a little toward its valid solution. Let us proceed with these.

When a line of activity is proposed, whether by an individual for himself or by a government for the society which it administers, theoretically there are two forms of planning possible. One is based upon the assumption that all the processes involved are known, as well as all the courses which they would, if unimpeded, spontaneously take. The plan simply adapts itself to these courses and thus cannot fail to be successful, because it is built upon the natural movement of the processes controlled by unbreakable natural laws. In other words, it is a planning based upon two main assumptions: first, *that there are definite natural laws and an iron determinism which resides in the social processes, and that a perfect knowledge of these laws is possible.* Social planning, therefore, according to this theory, can be thoroughly scientific. It does not require any independent factor of a special sort for its realization.

There is a second form of social planning, based upon somewhat different assumptions. It also demands some degree of knowledge of the natural phenomena involved, but unlike the first form it bases itself upon the assumption neither of a rigid determinism in the field of socio-cultural phenomena nor of an exact and perfect knowledge of these phenomena. What it believes in especially is *the all-important role of human volition and effort.* Whenever these factors enter into a field containing a variety of possibilities, their power is of the highest importance in determining both the nature of the plan and the degree to which the intended result becomes a reality. In extreme cases the partisans of this theory of volitional effort go so far as to believe in the possibility of realizing the normally impossible. The first type of planning, based upon the dogma of determinism and perfect knowledge, is in a sense *intellectualistic and passive:* it does not demand any particular effort except that which is present naturally and spontaneously. The second is *active and volitional,* based upon the explicit or implicit admission of something indeterministic, either in the sense of the existence of many potentialities of which only that one is realized which is backed by a special effort and determination, or in the sense of the existence of a wide margin of leeway between what is possible and what impossible: how closely the plan can approach the limit of the absolutely impossible depends upon the determination and effort of the planners.

Such are the theoretical types of planning and their assumptions.

In reality almost all planning represents a mixture of these types but with different proportions of the intellectualistic and voluntaristic elements. Some are nearer to the passive-intellectualistic, others to the voluntaristic type.

Thus we see that social planning may involve at least four different assumptions: first, the dogma of the determinism of socio-cultural phenomena; second, a belief in the possibility of a perfect knowledge of all the phenomena involved in planning; third, a belief in a relative indeterminism with reference to the phenomena in question; and fourth, a belief in the great power of the volition, determination and effort of the planners. Is any of these assumptions unquestionable and certain? Can they provide a firm foundation for the construction of a vast structure of realizable social plans and scientific "social engineering"?

1. As to the conflicting assumptions of determinism and relative indeterminism, both are of uncertain validity. My study of the comparative influence of these two theories from 580 B.C. to A.D. 1920 shows that they alternate in their rise and decline and in their domination at any one period: for instance, from the sixth century A.D. to the end of the twelfth century indeterminism dominates over determinism, while at other periods, for instance from the seventeenth to the twentieth century or from the third through part of the first century B.C., determinism has the upper hand over indeterminism.[2] If in the nineteenth century the deterministic credo was particularly fashionable and fairly generally accepted as the last word of science, such a credo in the light of a longer perspective appears just a "mentality-fashion" which like any fashion comes, blossoms and fades away. At the present time even in the physico-chemical sciences a belief in an iron determinism is over, melted down into a mere theory of probability. In the field of socio-cultural phenomena the variability has always been so great that we have virtually no rigid formula of the association, correlation, causal and functional dependence of two or more socio-cultural variables which can be accepted as definite, universal, eternal, free from exceptions, reservations, qualifications, and other limitations. We surely do not have any "scientific law" in a proper sense that formulates the socio-cultural uniformities. Consequently, we are lacking any definite and

[2] The detailed data and the curve of the fluctuation of the comparative influence of determinism and indeterminism in the Greek, Graeco-Roman, and the Western mentalities and cultures, from 580 B.C. to 1920 A.D. will be given in my forthcoming work on *The Integrated Culture: Its Main Types, Its Life-Processes and Their Fluctuations.*

well-tested deterministic foundation that permits us to stand firmly in forecasting the future direction of the socio-cultural processes involved in planning. The deterministic assumption, therefore, can hardly help us much in "scientific planning," based as this must be upon discovering in advance the future course of the processes involved.

If, however, we try to lean upon indeterministic assumptions, the situation becomes still worse, because the very concept of indeterminism means a denial of any uniform or definite relationship between two or more variables. By its very nature indeterminism in its application to human affairs rules out of existence all causal and functional uniformities. In one case A may be followed by consequence B; in another, by C; in a third, by D; and so on. Therefore the presence of A does not permit one definitely to expect that it will condition the consequence B, and not C or D or Z. Under such circumstances no forecasting of the future upon the basis of the past is possible; hence there can be no scientific planning so far as this presupposes an adequate knowledge of the future direction of the processes involved.

Such considerations are sufficient to put aside both of the assumptions under discussion. These assumptions cannot serve as a reliable basis for any forecasting, or for any planning based upon such forecasts.

2. Turn now to the second assumption of the passive-intellectualistic planning, that is, to the belief in the possibility of an accurate and valid knowledge of the processes involved and of forecasting their future direction, regardless of deterministic or indeterministic dogmas. *Savoir pour prevoir* and *prevoir pour pouvoir* are the popular formulae expressing this belief. Is such knowledge possible? Does it exist? Do we have at our disposal any formula that satisfies the requirement, especially if we put away the deterministic dogma?

Let us turn for the answer, first of all, to so-called relevant facts. It will no doubt be readily granted that it is easier to forecast one's own behavior than that of somebody else; it also will probably be agreed that to forecast one's own behavior is easier for twenty-four hours in advance than for twenty-four days or months or years. Any social planning presupposes a knowledge of how large numbers of people will be behaving under a given set of conditions for a more or less extensive period, and what will be the socio-cultural consequences of their behavior. Let us therefore ask: Can we forecast with

accuracy our own behavior for only twenty-four hours? My study of this problem,[3] with the able assistance of Mr. Clarence Berger, offers material that answers the question to some extent. One hundred and six unemployed persons were asked to write on a special sheet each evening how they were going to spend the next twenty-four hours, listing as exactly as possible every activity during this time and the duration of each. On the next day, with the aid of special supervisors, they had to check how they had spent these twenty-four hours actually. For more than three months such forecasting and checking was continued, thus supplying us with material which extends for each of our subjects through at least 2,000 hours, and for all together through more than 200,000 hours. In a similar way they forecasted how each would spend the day after tomorrow, then the day one week hence, then the day one month from the day of forecasting, in each case checking, after the event, the prognostication with the actuality. The preliminary results show that, in the cases of prediction for twenty-four hours, the average deviation in the time actually spent from that forecasted was 305.43 minutes per day, or roughly five hours out of twenty-four. The deviation in the forecasts forty-eight hours in advance was still greater: 353.68 minutes per day, or roughly six hours per twenty-four. The deviation showed a further increase in the prediction for the day one month from that of forecasting: here the result was 494.81 minutes per day, or some eight hours out of twenty-four.

I need not stress the fact that the deviations are enormous; that none of the persons questioned has been able to predict his own behavior even with the remotest accuracy so far as the length of the time for each specified activity is concerned. A deviation of from five to eight hours out of every twenty-four is a very serious matter even in a train schedule, let alone human behavior. And, as we have seen, it increases as the period extends farther into the future. If instead of the deviation in the length of time predicted for a specified activity one takes another test of accuracy, namely, identity of the *kind* of activities predicted with those actually performed, the deviation becomes even greater. The number of cases in which persons either did not perform the acts predicted or engaged in acts which were unforeseen is considerably greater than the number of the cases in which prediction and performance coincided.

[3] It will be published as a special monograph, *Human Behavior in the Light of Time-Budget Study*. [A brief summary of some of the results will appear in an early number of this *Review*. Ed.]

Thus this study shows that one cannot predict accurately even one's own behavior twenty-four hours in advance! Or, one can predict and plan roughly from three-quarters to four-fifths of his activities twenty-four hours in advance and in the circumstances free from unforeseen "accidents" and "catastrophes." If we try to predict somebody else's behavior, the deviation is likely to be much greater. If we try to predict our own or somebody else's behavior one year or several years from now the error, as the data show, in all probability will notably increase.

Granted that the amount of the deviation in the instance of these one hundred and six unemployed is not necessarily typical of all social classes and groups; granted that it depends upon many conditioning factors, and particularly upon normality and orderliness of social life. Yet, as the other data of our study show, some amount of discrepancy between the predicted and the actual behavior will occur with all social groups, and with almost all individuals, and under all social conditions, increasing with the increase of disorderliness and the frequency of upheavals and "accidents" in social life.

If such then is the situation with reference to the prediction of one's own behavior and that for only a relatively short period, how much more difficult is any *accurate*[4] prediction of the behavior of vast numbers of people under the conditions obtaining in any large-scale social planning! To foresee *accurately* how every one of these millions would behave and what the results of such a complex of behaviors would be, under, let us say, the condition of the annihilation of private property, or under the NRA or AAA or the "Soak the Rich" taxes or with the elimination of the Gold Standard, is a task for a Super-Genius or a Divine Mind, not for the mind of mortals. Those who start such planning may delude themselves and others into believing that they foresee and know really what the results of a given set of measures are going to be; but one should not confuse such self-delusion (as happens quite frequently) with real knowledge.[5]

[4] I mean *accurate* prediction and planning. Even fairly ignorant and foolish planning (with the prediction involved) rarely fails to 100 per cent. In part it usually comes true.

[5] It may be claimed that prediction of the future of groups and organizations is easier than that of the future of individuals. A study of the life-duration and mortality of social organizations does not warrant this claim. When a social organization of whatever kind is started, it is started with the belief that it will endure and grow. Meanwhile the factual study of the life-span of social organization shows a terrific mortality among them. Small economic organizations like drug and hardware stores live on the average about 3 years; the average duration of life of the largest economic and business corporations is around ten years; of the literary and cultural organizations of a local character in the United States around 2 to 3 years,

The facts of history likewise offer endless evidence of the impossibility of any accurate knowledge of forthcoming events, especially in the more or less remote future, ten, twenty, or fifty years from the moment of planning. If somebody in 1914 had predicted a small part of the happenings since that time, he would have been branded as mad. And yet madder things have happened than were forecasted. With such surprises the cup of history is filled to the brim. If the socio-cultural and historical processes were merely mechanical recurrences like the revolutions of a motor, forecasters and prognosticators, social planners and engineers would long ago have had a basis for their prediction, their *savoir pour prevoir*, and, for the planners, *prevoir pour pouvoir*, the knowledge from which to plan. But alas! if in the nineteenth century such a mechanistic conception of these processes had some credit, nowadays only the mentally deaf and blind, like various of the pseudo-behaviorists with their rubber-stamped and meaningless stimulus-response, conditioned and unconditioned reflexes, can hold such a creed. If these socio-cultural processes are not absolutely new at any given moment but contain repeated elements, the recurrences are in any case ever-new variations on old themes. Therefore history has been and will probably continue to be rich in surprises which play havoc with all forecasting and plans, except with the few lucky ones which by chance happen to turn out right.

A further difficulty in the way of such forecasting and planning is our inability at present to secure reliable knowledge about the causal or functional relationship between the processes which operate in any piece of social planning. On what indeed can we rely in such prognostication? In the past men relied upon Pythian oracles, prophets and other persons or groups believed to possess such a clairvoyancy of the future either as a gift of God or of the devil. Viewed as emanating from a divine or supernatural source, this basis was considered unshakeable and perfectly certain. A variation of the method of prophecy was a set of magical and religious procedures—augury—which served a similar purpose and shared similar certainty. In some form or other such institutions have existed among all the or-

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and so on. Each case of mortality of an organization is in a sense an evidence of wrong forecasting and therefore poor planning. See the actual data in P. Sorokin, "Life-Span, Age-Composition, and Mortality of Social Organizations," *Mensch en Maatschappij*, 9e Jaargang: 69–85, 1934. These and similar relevant data concerning the growth and decline, changes in structure, functions, and destiny of social organizations and groups fairly convincingly contradict the above claim.

ganized nations, often have occupied an important place and performed fundamental functions. The case of Greece may serve as an example. Incidentally, all this indicates that social prognostication (forecasting) and planning are as old as human society. But our "scientific" planners cannot and do not want to rely upon such supernatural bases in their forecasting. They declare themselves above such superstitions.

Another method of forecasting (and planning) has been that of astrology, a method which has not yet disappeared from human society. It is true, for example, that in the form of the sun-spot, climatic, sidereal and geographic theories of business-political, social-cultural fluctuations, astrology persists, since many of these theories differ little, if at all, from most of the best astrological theories of the past, like those which Clode Ptolemy developed in his famous *Tetrabyblos*. However, most of the modern scientific forecaster-planners have been critical of such theories and are unwilling to use them.

What then remains? Functional and causal study of the relationship of the societal variables? Very well. But how are such relationships, if they exist, supposed to be discovered? Through experiment? This is impossible in 99.999999 cases out of a hundred of complex social configurations. Inductively? But any induction is possible only when all the variables involved remain constant and only the variable studied is changed by the experimenter. Such a situation rarely, if ever, obtains in fact in any configuration of important societal variables. The other conditions are almost never constant or equal. All or some of them change incessantly. New factors are constantly appearing, others disappearing; in addition the remaining ones fluctuate qualitatively and quantitatively. Under such conditions no really inductive method can be used, and in almost all cases no inference can be derived from a mere observation of the "facts" discovered by means of such "induction." Observation? But we know how unreliable it is for any generalization and formulation of uniformities. Statistical method? Coefficients of correlation and the like? If a few years ago, when such paraphernalia were a novelty, we believed in their mysterious power to detect functional and causal uniformities, nowadays only a few freshmen in scientific study and statistical method can persist in such a belief. We know that this "key" cannot be relied on to open the door to functional uniformities and causal relationships. If, occasionally, we seem to be successful,

it is merely by a lucky chance, which occurs independently of the efficacy of our statistical apparatus.

To sum up, we do not have any certain and unshakeable basis or method. More than that: when we have even its simulacrum, it gives at the best something very relative, something which can be expected only under specific conditions and even then only as a probable possibility and never as certainty.

That these considerations are not far from the truth is indirectly corroborated by the following hypothetical reconstruction. We know that the institution of forecasting in the form of oracles and prophets and augurers existed for centuries among many peoples, like the Greeks and the Romans. We know also that few important enterprises were undertaken, whether by the rulers or the other leaders of these societies, without the preliminary consultations with and predictions of these agencies. The very fact that such institutions could persist for centuries suggests that in their prognostication the percentage of right guesses could not be unsatisfactorily small. Otherwise the agencies would have been discredited quickly and then eliminated. At least, we have seen before our very eyes how quickly many of the supposedly scientific statistical agencies and publications for forecasting—especially in the field of business—were discredited after the crash of 1929 which sent all such prediction to the four winds. This suggests that perhaps the percentage of right *guesses* compared with the wrong ones was rather higher in the institutions like the Pythia of Delphi or the famous Apollo oracle than in the contemporary predictions done supposedly upon the firm basis of science. This illustrates also how unreliable may be our forecasting in the field of social phenomena, and how difficult it is to acquire a knowledge of the future direction of social processes.

Out of the many other reasons for which adequate foreknowledge is difficult to obtain in these matters, I shall mention only two more. One is what in one of my papers[6] I styled the "principle of limit" in the relationship of two or more social variables. Even when the existence of some tangible association between two variables is more or less probable, we rarely, if ever, know what are the concrete values of each of these variables beyond which it disappears or even becomes opposite. This alone makes forecasting (and scientific planning) exceedingly difficult.

[6] See P. Sorokin, "The Principle of Limit," *Publication of the Amer. Sociol. Soc.*, Vol. 26, 19–27, 1932.

The second reason arises from what may be styled the partial blindness of almost all social prognostication and planning. We may grant that in several fields *a few results* of the introduction of a new invention or a reform in law or the modification of some specific existing condition or other, may be guessed properly: for example, the elimination of open saloons and liquor stores as a result of the prohibition law, the increasing velocity of travel as a result of the introduction of the automobile, and the like. However, in these, as in the larger part of such processes, many—and sometimes the most important—consequences are rarely, if ever, foreseen. Few of the prohibitionists could predict, at the moment of the passage of the eighteenth amendment, that one of its consequences would be the creation of a great bootlegging industry and the crystallization of the gang form of criminality. If out of the theoretically possible consequences of a given plan-forecasting A, the effects B and C can sometimes be guessed rightly, the effects D, E, F, . . . *n* are not foreseen at all in most of the cases, and the total effect of these unforeseen consequences not infrequently turns out to be much more weighty than that of the foreseen B and C. The net result in such cases is a profound discrepancy between the expected and the actual effects, and such a discrepancy always means the failure of the plan.[7]

For these and many other reasons one has to be more than humble in claiming the possibility of accurate social planning on the basis of scientific foreknowledge. Those in the field whose claims are excessive are almost invariably either ignoramuses or charlatans.[8]

So much for the planning which claims to be based upon scientific knowledge of the phenomena involved and of their direction and constellation in the future. Turn now to the second variety of planners, the volitional variety.

3. So far as the volitionalists claim the possibility of foreknowledge of future social phenomena, all the above arguments are applicable to them. They therefore need not be repeated here. As to

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[7] This point should be stressed particularly strongly. Almost all of the planners parade and advertize the few results which actually coincide with those planned; but they always forget a host of the results that are neither planned, nor expected, and yet, actually occur. If and when they are considered, they give the reality quite a different appearance from what was planned and expected. In many cases, some objectives can indeed be realized; but their realization is followed by a legion of other consequences—unplanned and unforseen—which change—and often radically—the situation mapped in the plan.

[8] Comp. E. B. Wilson: "Are there Periods in American Business Activity?" *Science*, 80: 193–99, 1934. Also E. B. Wilson, "The Periodogram of Am. Bus. Activity," *Quar. Jour. Econ.*, May, 1934.

the purely volitional factor, one has to admit that it is indeed a most important force in the realization of any plan. *Sic volo sic jubeo* has been the central element in most of the planning. If there is a definite determination on the part of the planners, if their volition is followed by adequate and proper efforts, and if these efforts are more powerful than the volitions and unorganized efforts of the mass of the population, then many plans have a chance for success, provided that they are not entirely fantastic. To drive from my home to my office usually takes about fifteen or twenty minutes. But if I am late I make an effort to drive as fast as I can and I reach my office sometimes in ten minutes. If a dictatorial planner presses relentlessly and suppresses pitilessly all the opposition and eliminates all the opponents, he can put through, at least for some time, something of his plans. Communism, Fascism, Hitlerism provide adequate illustrations of this.

The nature of such planning makes clear all the hazards and uncertainties inherent in it, as well as its reckless and purely guessing elements. It is evident that it can achieve success only when the total sum of the determination and effort of the planners is greater than that of the efforts, determination and sheer social inertia which oppose them. Otherwise, the efforts of the planners would be wasted. In such a case the plan cannot be carried through; it is doomed to fail. What the power of the efforts in behalf of the plan is in comparison with that of its opponents, both conscious and unconscious, cannot be measured exactly, and therefore remains in most cases guess-work. Sometimes the guess turns out to have been close to the truth, sometimes not.

But even when the effort of the planners happens to be greater than that of the opposition, even in this most favorable circumstance the plan rarely, if ever, succeeds completely. Usually it ends in something abortive, and the real consequences of the plan are always different from the planned results. This is because of the above-mentioned principle of limit, and of the impossibility of our foreseeing *all the important consequences* of any plan or innovation. Voluntaristic planners especially are likely to be narrow-visioned or blind. Within a very limited *vista* they see their goal and concentrate all their efforts to reach it, without perceiving either the situation in the rest of the world that lies beyond or what the results of their efforts will seem to be from the broader point of view. Such planners are quite similar to ignorant persons who have to deal with a delicate piece of

machinery. Seeing that this or that screw or part is out of place, such persons may try to set it into its supposedly proper position, using whatever means they have available. The usual result is that the part can be forced into position, but that through the coercive force many other parts of the machinery are displaced or broken, and consequently the machinery is not improved but injured, even though the screw has been put into place.

In the past as well as in the present we have plenty of cases of this class. The classical instance is offered by the entire Russian Communist Revolution. The initial plan of the Communist Party, when they were fighting for power and at the beginning of their dictatorship, was that when the power was actually in their hands their dictatorship and its measures were to lead, according to their expectations, to the following results, among many others: to a rapid improvement of the economic situation of the masses; to the abolition of capital punishment; to the increase of freedom; to world revolution. What were the actual results in these directions? Quite the opposite: horrible impoverishment and starvation; hundreds of thousands of persons executed; complete elimination of freedom; and failure of the world revolution. The subsequent planning of the Communist regime was manifested in the five- and ten-year plans. Parts of the objectives of these plans have to some extent certainly been carried through: heavy industry was built up; farms were collectivized; the family was practically abolished; complete liberty of divorce was established—all according to the plan and often up to 90–95 per cent of the plan. In addition, other objectives were realized which need not now be specified.

But side by side with these realizations a host of other results, unforeseen and unexpected in the plan, came about, and these results have been so heavy in their import that they have wrecked the plans fundamentally. For example: neither the five-year plan nor the ten-year plan reckoned with the depreciation of Soviet money. And yet, during the first three or four years of the five-year plan the Soviet rouble fell in purchasing power from a value of some forty to forty-five cents in 1928–29 to a value of three to five cents. The plans did not take into account the decided impoverishment of the masses which resulted, impoverishment to the point of mass-starvation with its millions of deaths. And yet, these terrible results occurred, and the welfare of the masses fell to the lowest possible level; in 1933–34 alone from three to seven millions died by starvation. The plan did not an-

ticipate that for the collectivization of farms it would be necessary to shoot and banish from two to four millions of the most industrious peasant families. And yet this happened. The plan did not foresee that from one-third to two-thirds of the farm cattle would be killed within the first year of the plan by the coercively collectivized peasants. And yet they were killed; and even now the number of cattle is still below what it was before the collectivization. The planned Soviet family reforms did not foresee either the millions of wrecked lives of girls and women, or the from ten to thirteen millions of wild urchins, hungry, cold, diseased, undisciplined, uneducated, running like wild animals in the streets and in the country and perishing by thousands from the lack of all the necessities of life and of any care. And yet this happened. The plan did not take into account the enormous decline in the efficiency of the work nor the enormous mobility and turnover of the workers. And yet this happened. One of the results of the planning now, after seven years, is that even the purely economic standard of living of the masses is still lower by roughly one-half than it was before the Communist revolution. On the basis of the Soviet data the average real wage of the working man in 1934 was about twelve gold roubles or about six pre-Rooseveltian dollars a month, while before the Revolution it was between twenty-two and twenty-five gold roubles.

In brief, hundreds of unforeseen results have come about, results which are so weighty and so contrary to those expected that, in spite of the millions of lives sacrificed, in spite of the indescribable sufferings of millions paid for it, the plan is wrecked: if along a few lines its expected objectives have been carried through, in many more directions hosts of results have occurred which in their totality have made a caricature of the plan, a kind of tragic mockery. I mention this not for the purposes of blaming or praising the Communist planning experiment—I leave this to the sense or nonsense of the reader; but simply as a conspicuous case of the deviation of the actual results from the predicted and planned. Such a deviation is unquestionable and the few facts indicated are perfectly certain. Planned effects A and B are achieved; but together with them a crowd of other effects: C, D, F, . . . *n* came unforeseen and uninvited.

With proper modifications the same can be said of most social planning on a large scale, both past and present, whether it be the NRA and AAA; the plans of the Italo-Abyssinian war of Mussolini; those of the Third Reich of Hitler; or the plans of the Townsends,

Huey Longs, Technocrats, and other political or economic or scientific visionaries. If and when all the actual consequences of a plan—A's and B's as well as D's, F's . . . and *n*'s—are considered, I do not know any single case of large-scale social planning, present or past, which succeeded in even fifty or sixty percent of its objectives, to say nothing of complete success. And the larger the scale of the planning the greater has been the discrepancy between the actual and the expected results.

To sum up: Since socio-cultural life changes incessantly, planning is unavoidable as an adaptive reaction to these changing conditions. In this sense it took place in the past, goes on at the present, and will continue in the future. But from this unavoidable necessity it does not follow that any such scheming will be successful or that with the passage of time the percentage of successful planning (i.e., where the expected and actual results coincide) will increase, or that it has become so much more "scientific" that we have a right to boast of our ability to forecast and control socio-cultural phenomena. At the present, all such schemes remain as much guess-work and gambling as they were in the past. Only in recklessness perhaps does our present planning abound. We live indeed in an age of the most reckless planning, in an age of adventurous gambling with the life and values of millions of human beings. For this reason it is perhaps appropriate to finish these considerations with a humble plea to all over-enthusiastic planners: "Please, go on with your game; but better underdo than overdo your gambling. Still better, if you can, study carefully the phenomena you are going to engineer before engineering them; still better, if you can, try your plan experimentally, on a small scale, before recklessly starting it on a large scale and in earnest. If you see that in the experimental setting it does not work, you will readily be able to abstain from its actual application to social life."

DISCUSSION

HENRY PRATT FAIRCHILD
New York University

THE VIGOROUS and stimulating paper just presented by Professor Sorokin seems to me to be marred by two or three confusions that interfere with clarity of thought and analysis. Professor Sorokin affirms frankly his belief that social planning is possible—that is

some social planning, but he denies that social planning can ever be 100% perfect in its results. On this point, I am in complete agreement with him. The real question is as to the extent to which social planning can be effective, and here is where my basic disagreement with him lies.

The first serious difficulty with Professor Sorokin's paper is that he confuses "social planning" with "social prediction." These two concepts are fundamentally different, in fact practically opposite. Social prediction assumes the operation of constant social forces which produce consistent and continuous results, representing unbroken sequences in such a way that the future can be known from the past. Social planning, on the contrary, assumes the special interposition in social relations of human will and purpose, in order to deflect or divert social movement in such a way that the future may not only be different from the past, but different in ways specifically visualized in advance.

With respect to social prediction, it has long seemed to me that any grounds for doubt have been removed. Our whole practical life is based on social prediction. Many of you in this room are staying in hotels here in New York City. When you got up this morning you turned on a tap marked "Hot" and predicted that hot water would flow. You entered the breakfast room and ordered a meal from the menu, predicting that each item would arrive in practically the form that you desired. These predictions are essentially social, because their fulfillment depends on the coöperation of hundreds and thousands of individuals scattered literally over the entire surface of the globe. And so we might go on through the whole day, recognizing that practically every important act was in some degree a social prediction.

Another distinction which Professor Sorokin fails to make clear is that between "social planning" and "societal planning." Here again, it seems to me that there can be no reasonable disagreement as to the possibility of social planning. Instances abound on every hand of positive achievements resulting from the deliberate efforts of groups of people to direct social change, and to create new social forms and constructs.

This meeting itself is an excellent illustration of social planning. It may not be a 100% perfect meeting, but it is a very good one. There may be some individuals who intended to come who are not here, but the attendance is about what was expected, and I have

heard of no important paper which failed to be presented. On a somewhat wider scale, we need only cite the great victories in the practically complete control of such diseases as typhoid and diphtheria and the great reduction of tuberculosis. These are fully as much sociological achievements as they are medical or public health. I often think that we sociologists are far too modest in laying claim to the practical accomplishments of our own science.

If we turn to societal planning, of course the problem becomes much more difficult. Here we are dealing, not with localized groups or limited special interests, but with the whole structure and functioning of the community itself. To achieve results on this scale necessitates the purposeful direction and control of many, if not most, of the major social impulses and drives. To expect even approximate perfection would of course be absurd. But we need not be deterred by the probability of minor failures, and we should not be dismayed if the practical results fall considerably short of our wishes or our anticipations. To be sure, social science does not yet operate with the same precision as the physical sciences, and yet even in the latter field the results of planning are not always perfect. Take for example the great steamship *Normandie*. In the planning and construction of this vessel, we may assume, the very best technical and engineering resources of France were assembled, and every possible contribution of science was utilized. And yet the result contained a large element of failure. As we know, the ship has been laid up several times for alterations in the hope of correcting serious defects. It is said that the passenger on this liner need only drop the sugar into his coffee and the ship will do the rest, and I have been told from an authoritative source that it is impossible to freeze ice cubes in the refrigerators, because the vibration jiggles the water out of the trays.

On the other hand, if we wish to recognize a notable instance of essential success in societal planning, we need only think of our own country. Who can question the fact that the form and structure of the United States are the result of a definite plan conceived a century and a half ago by a group of specially commissioned individuals, and embodied in a document known as the "Constitution"? Doubtless this great structure has not functioned with complete smoothness, the Constitution has been changed a number of times, and some of us believe that it might very well be pretty thoroughly remodeled to meet modern conditions. The Framers had no sociology to guide

them. But nevertheless the country has thrived and developed under this instrument, and has proved to be about the sort of a community that the founders visualized.

With reference to the Soviet Union all I can say is—and I say this very sincerely after having spent the entire summer there—that, far from sharing Professor Sorokin's view that it is a lamentable failure and a proof of the futility of social planning, I regard it as an amazing and impressive body of evidence that a determined, rational, and intelligent societal plan may work out in very nearly the manner that was intended.

SOME CONTRIBUTIONS OF SOCIOLOGY TO THE GUIDANCE OF SOCIETY

E. A. Ross
University of Wisconsin

I

Sociology should be able to provide full and reliable information as to *the makeup of the social population*. By this I mean: composition in respect to race, sex, age and national origin; proportion of adults single, married, widowed or divorced; ratio of the urban population to the rural; rates of births, deaths, marriages and divorces; literacy; religious affiliation; proportion in school; distribution among the occupations; inmates of disciplinary and penal institutions; distribution of income, of property, of taxes. In some cases the mere injecting of precise unquestionable data on these matters puts an end to controversies which otherwise might drag on forever. Change of certain of these measurements in the wrong direction will rouse thinking people like an alarm bell. Just as change in temperature, pulse, blood pressure or white-corpuscle count may be highly significant as to a man's prospect of health or disease, so change in marriedness, tenancy, proportion of infants, or crime rate may disclose the existence in society of a pathological trend which calls for prompt remedial measures. Every advanced society should be provided with a statistical bureau to furnish reliable measurements of all social facts and quantities to which sociologists attach great significance.

II

Sociologists ought to recognize it as a part of their job to find *at just what places the shoe is pinching*. If there is anywhere among us a considerable element of normal persons who, in spite of industriousness, sobriety and thrift, are unable to make a decent living, that element should be brought to public attention as presenting a problem. Our country is not overpopulated, therefore the persistence of such an element demonstrates that something is wrong; thorough knowledge of such an element ought to sweep away the various false

hypotheses as to what the root of its trouble is and make clear what sort of remedy is called for and at just what point it ought to be applied.

III

By providing trustworthy data with respect to *interest groupings* —corporations, coöperatives, labor unions, credit unions, chambers of commerce, consumers' bureaus, women's clubs, political parties, and other pressure organizations—sociology should make it possible to gauge the comparative strength of crystallized or organized social forces. We ought to be able to perceive which elements in society are actually in the saddle and which are being ridden over. Coming social strains and conflicts usually "cast their shadows before" in the form of great crowds and ovations for the propagandist who adequately voices what great numbers of aggrieved or dissatisfied persons are thinking or feeling. A hurricane growth of clubs to attain a specific end by a definite means signifies a widely felt need and should warn the statesman to devise a remedy, if he wishes to be kept on at his post.

IV

By unbiased description of all the important social experiments which bear upon the difficulties encountered in contemporary society the sociologist should be able to disclose unexpected reactions and behavior tendencies, which the would-be reformer will have to take into account. Review of the outcome of a great variety of social experiments shows that nowadays lack of original and appealing ideas is the last thing our society suffers from. What it needs is new and beneficent ideas *which will really work, which will actually fulfill expectations*. Many social experiments which at first excited glowing hopes have turned out disappointingly because people did not react to them as had been anticipated; other experiments, e.g., consumers' coöperatives, have succeeded and spread. Since proposals which have a record of consistent failure after repeated tests crop up again and again, owing to general ignorance of the facts, sociology should be prepared to report upon the results of all manner of significant social experiments. It goes without saying that experiments should be observed and evaluated not by their enemies, but by their well-wishers; but they must not be so enamored of a particular remedy that they are unable to perceive its failure in case it turns out badly.

V

Sociology does not see the future of society as *predestined.* It rejects the argument of Karl Marx that, seeing that the bourgeois class overthrew the feudal social order, the working class is bound to overthrow in turn the bourgeois social order. To Marx it seemed to be "written in the stars" that the proletariat will triumph and bring an altogether different social order; the only room he recognized for the operation of the human will is in the *date* of this overthrow. If the proletariat should develop a will-to-power and be well led, the new social order would arrive so much the earlier.

Sociologists reject such assumptions. As we see it, the feudal order disappeared because the forces supporting it came to be much weaker than the forces assailing it. Capitalism will be superseded, *if it is superseded,* for the same reason. The forces supporting and the forces assailing capitalism by no means match up, point to point, with those which supported and those which assailed feudalism. In some respects the defenders of capitalism are weaker, in other respects they are stronger, than the defenders of feudalism. Likewise, the assailants of capitalism are not made up of the same elements nor do they wield the same weapons as those who overthrew feudalism.

Therefore the issue of this latter-day conflict is by no means a foregone conclusion. The Marxian leader cheers on his followers with the slogan, "Our victory is written in the book of Fate. We are bound to win." The sociologist observes, on the other hand, that the outcome of the duel between *private capitalism* and *communism* depends upon the balance between the contending forces, and these in turn depend upon developments, *many of which have not yet taken place.* The eventual collapse of the feudal social order was visible to some as much as two hundred years before it actually took place. Now, granting the inevitability of collectivism, is its triumph a matter of two or three decades only? Or are we in the presence of a social system which will not become utterly effete for two or three generations? How this question is answered makes an immense difference in our attitude toward Communism regarded as a contemporary political movement.

The sociologist may be able to discern that certain forces are gathering momentum while others are getting weaker, that one side is winning ground whereas the other side is losing ground, but, until he knows more than he knows now, he cannot be sure that these tendencies will continue. He cannot but feel that prophecy as to the

ultimate outcome would be hazardous. Whether one day sociology will be able to trace the path *which society will surely follow*, just as the geologist confidently predicts what the borings will show as to the mineral resources half a mile underground, I am not prepared to foretell.

VI

During the last twenty years sociology has done much to dispel the fog which wrapped the subject of social revolution. Real advance has been made in ascertaining the conditions under which revolutionary movements arise and in charting the course which they are likely to pursue. In a few more years we ought to reach the point of successful *social prognosis*. I mean that, just as a good physician after examining a person can declare with positiveness, "You stand in peril of pneumonia," so before long a good visiting sociologist may be able to examine a society for a few months, then confidently emit the warning, "You are in danger of a serious revolutionary outbreak."

VII

Sociology takes the dispassionate, scientific view of tradition. Through most of the past there were conservatives praising tradition as the chief guide of life and norm of social policy, while progressives denounced it as the greatest obstacle to social progress. We now see that each of the two is right some of the time, and that both are wrong in some respects. Before the rise of psychology and social science, tradition, embodying the fruits of the experience of bygone generations, had some guiding value so long as the underlying social conditions had not greatly changed. But, in a time like ours, when new inventions, new techniques and new insights are making many "lessons from the past" inapplicable, the thoughtful will prefer to look to science for guidance rather than to tradition.

Since, in a time like ours, tradition cannot afford safe guidance, because much of what it has to offer really has no bearing on the matter in hand, the only thing we can fall back upon is popular intelligence. Since this, once it recognizes the futility of tradition, finds nothing it can put its trust in save education, I echo the saying of H. G. Wells "The future depends upon the outcome of a race between education and catastrophe."

DISCUSSION

F. H. HANKINS
Smith College

IN THE BACKGROUND of the question whether the sociologist, as sociologist, should enter the field of practical affairs and, if so, what he should do, are certain large issues of social theory which can profitably be discussed first. The basic questions are whether man can do anything to control the evolution of himself as a genetic product, of his society as a group, and of his culture as the medium and means. If we reach a clear affirmative answer to this question, we must in so doing reveal the *conditions* which make control possible. We shall then be in a position to consider the *extent* to which control in any genuine sense is feasible at any time.

To begin with then, we accept as a necessary starting point the deterministic view that every event has its causes in intrinsic factors contained in the stream of events. We might suppose that this is taken for granted as a working hypothesis by every scientific person were it not for the fact that we now and then find evidence to the contrary. The actions of the individual fit into a chain of causally connected events, and millions of such chains constitute the processes of cultural evolution The rather barren notion that culture evolves automatically will have to be discarded from this viewpoint. If culture rolls along from one phase to another by virtue of the forces operating solely on an objective, impersonal cultural plane, then we might as well resign ourselves to our fate, and say "Let her roll, and be damned!" We must then bid good-bye to all fruitful study of social psychology, social structures and cultural processes. The fact seems to be that culture as a sort of independently existing set of phenomena is a pure mental abstraction, much like social mind. Not only is culture solely and altogether a product of human behavior, even when we look back on what we call a past culture; as a vital evolving scheme of social life and organization in the present it is also solely and altogether composed of human behavior. Culture patterns *exist* only in human behavior; as do also social institutions.

The evolution of culture, therefore, is accomplished through human behavior and this behavior is firmly imbedded in the steam of cultural evolution itself. Putting the matter thus brings out clearly the fact that what the individual does has some effect on the trend

of cultural evolution. What the mass of individuals does constitutes the cultural evolution itself. But, you will say, what any individual does and thus what they all do, whether viewed individually or collectively, is determined by antecedent and coexisting events. True enough; but it must be remembered that a large part of the social forces involved in social evolution are humanly determined; they represent human capacities and behavior patterns. We do not lose our freedom when we act according to our own natures, even though that nature is antecedently determined, because ability thus to act is the only tenable meaning of freedom.

On the deterministic plane, therefore, we can find some basis for genuine hope, because we can acquire scientific knowledge and make use of it to control and adapt, so as to realize human objectives. We have the assurance we can go places, (1) if we get the knowledge of how to get there, (2) are able to apply that knowledge effectively through group action, and (3) put forth the effort necessary to make the journey. There are thus three categories of knowledge. The first, noted above, is knowledge that our efforts have effects on cultural evolution, because our behavior constitutes such evolution itself. Knowledge alters conduct; the more intelligent we are the more it does so. But knowledge that effort has effects is only an encouragement to effort; of itself it will not carry us far. We can only achieve our objectives when we know their causes and have control over such causes.

This second kind of knowledge, the scientific knowledge which gives power, we have in abundance in various directions. True enough, whether man has the will and the mental capacity to exert himself, so as to acquire and use this knowledge, is a part of the causal stream of wholly determined events described in the rise and fall of civilization; but no other conception of the matter gives even a jot or a tittle of human control over human destiny. There are those[1] who see an end of what may be called the present scientific era in about two hundred years. If they are right, the present devotion to science will culminate and then decline, giving way to a new age of witchcraft and necromancy.

Then there is a third kind of knowledge, which in practice cannot be separated from the arts of political manipulation and some philosophy of social welfare. This is the knowledge of how to put into operation the conclusions of scientific research through the agencies

[1] Eric Bell, *The Search for Truth*, Williams and Wilkins, 1934.

of social control. An all-wise and benevolent dictatorship could accomplish this miracle, but whether it can be done through democratic institutions is extremely doubtful. In any case, we need have no illusions as to the possibility of complete social control—the conscious social telesis of the late Lester F. Ward; that seems definitely ruled out.

Our next preliminary inquiries may well be, "How much knowledge have we got, and how far will it carry us toward the solutions of important problems?" Technical knowledge in the physical sciences is now obviously sufficient to provide an abundance of material resources in western nations. Moreover, the methods of acquiring more such knowledge are well developed, as are also the arts of applying such knowledge for private ends. It is in the field of the social sciences that knowledge is most meager. We have stray bits of knowledge that are helpful in influencing developments here and there, but with reference to the major aspects of social life as a whole we must acknowledge drift rather than mastery

You will say that schemes for the rational solution of these problems are already available, but I am sceptical. It is easy enough to construct a closet utopia; to construct a real one is a different matter. A social plan may appear to be rational in the sense of logical. This would mean only that its basic assumptions as to causal relations were carried out coherently. To be fully rational a plan must be based on scientifically established knowledge of all the actual causal processes involved in its realization. To be practical a plan must also include control over those processes. We have very incomplete knowledge and even less control. Professor Ross notes that "many social experiments have turned out disappointingly." Nevertheless, he adds that "Experiments should be observed and evaluated not by their enemies but by their well-wishers," which sounds suspiciously like the "stand-pat" Republican slogan, "Revision of the tariff by its friends." Not only must experiments be scientifically evaluated as regards means to ends, but (1) their ends must also be evaluated as aspects of a philosophy of social welfare, and (2) we must have both the knowledge and the art of social control to put them into effect.

The certain continuance of social improvement thus requires at least four things. First, agreement as to what constitute the supreme social values; secondly, knowledge of what kind of social organization and institutions are most likely to secure them; thirdly, knowl-

edge of the causal processes whereby such organization can be established and maintained; and fourthly, actual social control over these processes.

We are obviously very far from any such possibilities, and will long continue to be. Instead of being skillfully directed by social engineers equipped with clear insight into the social processes, the social system will continue to evolve. All we can possibly do is to give it slight impulses here and there, for better or for worse. It may be true, as Professor Ross boasts, that a visiting sociologist could prophesy a revolution; this assumes that the visiting sociologist would be listened to, which I very much doubt; and in any case proves only that prophecy is easy. If the revolution came off more or less according to schedule, the sociologist could say, "I told you so." If it didn't, he could argue that his warning set going counteracting forces and thus saved the day.

Coming then to the immediately practical aspects of the matter, I think it highly probable that we as sociologists will go on doing what we have been doing. The first desideratum is more knowledge, as everybody will agree. This knowledge is by no means to be acquired solely by the processes of research most commonly applied, which consists of sinking post holes here and there in the vast field of social phenomena. This is primary, however, and I would not be classed among the critics; nevertheless, on the whole, it is subject to less ultimate difficulty than the synthetic interpretation of related bits of knowledge from the entire field of social inquiry, in relation to social objectives and values.

Next comes teaching. Here the objectives are the spread of knowledge, the cultivation of sceptical minds, and training in habits of sound reasoning. Professor Ross finds nothing he can depend on save education, and sees cultural evolution as "a race between education and catastrophe." To a large extent I agree; but my feeling about education is not as hopeful as it once was or as it still is in our tradition. This view assumes that the population mass in a great democracy can be educated sufficiently and rapidly enough to enable them to pass intelligent judgment on public affairs. This I seriously doubt. I see a race between the civilizing forces and dysgenic reproduction; but then, in these matters, each of us is bound to emphasize his own hobbies. In any case, so far as it goes, education is indispensable. It at least reduces the number of utopians among sociologists; but the great Demos seems to me less and less able to

grapple realistically with the increasingly complex problems of western culture. If this be true, then the prospects for the application of social science findings will not be any better in the near future than they have been in the recent past.

Finally, among practical activities may be mentioned the entrance of sociologically trained persons into every possible aspect of social life where their knowledge and training may prove useful. This aspect of the matter has been admirably covered in the recently published report of the Society's Committee on Opportunities.

QUANTITATIVE METHODS IN SOCIAL PSYCHOLOGY

GEORGE A. LUNDBERG
Columbia University

THE TITLE of this paper represents a small subdivision of one of the subtlest problems in the field of sociology, namely, the nature of the symbolic mechanisms through which man deals to an increasing degree with his universe. These mechanisms consist of neuro-muscular sets of some kind in the human organism. When these sets, namely our verbal mechanisms, correspond closely to the conditions to which we must adjust, they greatly facilitate our adjustments. For example, a map is a highly valuable symbolic representation provided its pattern, order, and sequences correspond to the actual terrain over which we must travel.[1] It is a corresponding handicap, if it fails to indicate where the rivers and the mountains are, if it confuses the order in which they occur, or otherwise fails to correspond to the conditions to which we must adjust. Sometimes a local map which may be adequate for most kinds of travel within its own borders turns out to be very misleading in the light of a larger perspective. In the same way the verbal systems and orientations of a primitive primary group society may turn out to be grossly inadequate in a national or world society of secondary group relationships.

The best illustration of this phenomenon is the successive intellectual revolutions that mark the epochs of science. Each resulted from the difficulty of forcing increasingly adequate observations into the then existing verbal schemes. The new orientations, such as those of Copernicus, Newton, and Einstein, provided a more adequate intellectual chart according to which mental operations could proceed without contradicting the concrete observations of life. As Poincaré says: "All the scientist creates in a fact is the language in which he enunciates it."[2] Now social psychology is the branch of sociology which is increasingly concerned with the linguistic behavior of man. The present paper deals with one type of this linguistic be-

[1] *Cf.*, A. Korzybski, *Science and Sanity*, N. Y., Science Press, 1933, p. 58.
[2] H. Poincaré, *The Foundations of Science*, N. Y., Science Press, 1913, p. 332.

havior, namely, the transition to quantitative symbolism in the domain of social psychology.

I

One interesting by-product of this transition is the necessity of relying increasingly on written rather than spoken symbols. Many of the things that scientists wish to communicate simply cannot be adequately transmitted through oral language. The structure of the idea which scientists must communicate is so complex that it cannot be matched in a succession of acoustic stimuli. Consequently, the language of science must increasingly consist of written graphic symbols, which provide an enduring instead of an immediately vanishing stimulus, and offer possibilities of arrangement (tabulation, etc.) that cannot be communicated in oral language.[3] Largely for this reason, you will find the solid material underlying the present paper referred to in the footnotes.

The dominating intellectual chart according to which Western man has attempted to orient himself in the social world is the Aristotelian laws of thought laid down some 2300 years ago.[4] Physical science has gradually abandoned these rules of mental procedure but the social sciences still hold to them. One of the reasons for their popularity is the clean-cut dichotomies which they set up.[5] Propositions are either true or false, things are either right or wrong, and so forth in all matters. This principle resulted in dichotomizing nicely the facts of experience into mutually exclusive compartments, which have for centuries handicapped thinking by setting up categories assumed to be inherent in the universe and holding to them regardless of how badly they served the solution of the problems at hand.[6]

Illustrations of the prominent but mischievous role which these dichotomies have played in the history of human thought are too well-known to require more than mention. For example: (1) The induction-deduction controversy is, in the light of modern psychology, simply obsolete. (2) The heredity-environment controversy is

[3] *Cf.*, Leonard Bloomfield, "Linguistic Aspects of Science," *Philosophy of Science*, October 1935, pp. 499–517.

[4] For a good brief discussion of this subject see E. T. Bell, *The Search for Truth*, N. Y., Reynal and Hitchcock, 1923, Chap. VII. For extended discussion see Korzybski, *op. cit.*

[5] The arbitrariness of this system has been exposed in the epoch-making work of Lucasiewicz and Tarski; see C. I. Lewis and C. H. Langford, *Symbolic Logic*, New York, 1932; also A. F. Bentley, *Linguistic Analysis of Mathematics*, Bloomington, Ind., Principia Press, 1932.

[6] Curiously enough, quantitative techniques are suspected of being guilty of just this shortcoming, though the history of mathematics and statistics clearly demonstrates the opposite.

another case in point.[7] (3) Further illustrations are found in many of the arguments regarding structure *versus* function, organization *versus* process, form *versus* activity, etc. What is overlooked is that a structure is merely a persistent function, while a function is merely a series of changing structures.[8] (4) The arguments about case studies *versus* statistical method, to which this Society has given much attention, likewise disappears upon the reflection that all statistics necessarily consist of cases and that therefore there can be no antithesis or mutual exclusiveness between the two methods. The scientific import of the most thorough genetic or case or configuration analysis, on the other hand, lies in the demonstrability of its generality. The attempt to contrast quantitative techniques with theory is likewise fallacious, because quantitative approaches may be as theoretical as any others. (5) Other dichotomies of the same kind must simply be attributed to rhetorical rambling, which even the guilty authors would disavow if confronted with them. Take for example this statement of a prominent sociologist in a recent book: "The social sciences," he says, " . . . must be established through critical thinking rather than through physical science methods."[9] The implication is that critical thinking is less necessary in the physical sciences. As long as we obfuscate our thinking by such contrasts, no progress toward the solution of real problems is possible.

II

The futility of the antitheses mentioned above is so obvious that very few informed people are interested in the further discussion of them. In short, they are in process of being relegated to the same limbo of human interest to which we have consigned other famous specimens of scholastic argument. One or two others of these antitheses which are still rather prominent in sociology should be tagged with the same label of scholastic verbalization. I refer to the current attempt to contrast statistical, quantitative, and mathematical methods on the one hand and a method called the method of insight on the other. Sometimes such terms as understanding or intuition are substituted for insight. The error lies in overlooking that insight

[7] For elaboration of this point see G. A. Lundberg, "The Biology of Population Cycles," *Social Forces*, March 1931, pp. 405–408.

[8] L. K. Frank, "Structure, Function and Growth," *Philosophy of Science*, April 1935, p. 213; also Alexis Carrel, "The New Cytology," *Science*, Vol. 73, March 20, 1931, p. 298; also G. E. Coghill, "The Neuro-Embryologic Study of Behavior: Principles, Perspective, and Aim," *Science*, Vol. 78, August 18, 1933, p. 137.

[9] C. A. Ellwood, *Methods in Sociology*, Duke University Press, 1933, p. 5.

and understanding are the ends at which all methods aim, rather than methods in themselves. Quantitative techniques are merely the more refined, easily-used tools by which we gain insight and understanding.[10] No one has to my knowledge ever questioned their importance in all scientific endeavor. Eloquent defenses of insight and understanding, therefore, merely draw a red herring across the trail of the real question, namely, What are the methods of attaining understanding and insight?[11] We want an objective description of the *technique*. The answer to this demand in some quarters is to wear one's collar backwards, to gaze into crystals or tea cups, or to go into a trance. While being duly impressed with the remarkable results of these techniques, the hardier minds in every field have always demanded a more detailed description of the steps in the procedure. Some of the abler magicians, such as Houdini, have acceded to this demand by describing their techniques in verifiable terms. Since verification by other qualified minds is the essence of scientific knowledge, the progress of science has been characterized by increasingly searching demands that the author of a generalization specify the steps by which he reached it. Because of the essentially quantitative nature of all *scientific* generalization (I shall return to this point later), an honest description of the technique by which any understanding or insight is achieved will either involve quantitative procedures (formal or informal) or be of no general scientific import.

Take, for example, the phenomenon of prediction. In prescientific terminology it is called prevision and is generally conceded to represent the highest measure of insight. Is there any *scientifically accredited* way of predicting, except in terms of probability based on past observations? The whole argument is a confusion of language. Statisticians themselves fall into the confusion when they point out that the technique of correlation must be used with understanding, logic, reason, etc. They overlook that the understanding, reason, and logic which they properly advocate is itself a method, a technique, of some kind. In fact, the insight and the understanding which we

[10] Even crude and inadequate quantitative techniques sometimes reveal relationships which are not otherwise perceptible. *Cf.* L. L. Thurstone, *The Vectors of Mind*, University Chicago Press, 1935, p. 206. I am here accepting the conventional usage of these terms as describing an adjustment achieved, namely, when our curiosity for some reason comes to rest. Later I shall show that from the operational viewpoint *understanding* and *insight* must themselves be regarded as techniques. See footnotes 16 and 29.

[11] "The gestalters have made much of the notion of insight, but, unfortunately, this idea has become enveloped with a kind of mysticism. It appears to me that *insight* is only another name for a process which has long been familiar to us." O. L. Reiser, "The Logic of Gestalt Psychology," *Psych. Rev.*, Vol. 38, No. 4, July 1931, p. 360.

seek is to be achieved only by further correlation—formal or informal. Correlation is not merely the name of a certain statistical operation invented by Karl Pearson. It is, as the dictionary says, "the act of bringing under relations of union, correspondence or interaction; also, the conceiving of two or more things as related."[12] As such, it is a method used as frequently by other people as by statisticians. Misuse of certain specific correlation techniques for purposes for which they are not adapted is, of course, common, and nobody defends such errors, least of all the statisticians. At the same time, formal correlation is no more frequently erroneous than the informal correlations which everyone practices. To attack correlation and quantitative techniques in general, because of numerous faulty examples of their application, is a flagrant case of throwing out the baby with the bath.

At this point my analysis will be challenged on the ground that I interpret the meaning of statistics and quantitative methods too broadly. The current idea seems to be that if one uses pencil and paper, especially squared paper, and if one uses numerical symbols, especially Arabic notation, one is using quantitative methods. If, however, one discusses masses of data with concepts of more or less instead of formal numbers, and if one indulges in the most complicated correlations but without algebraic symbols, then one is *not* using quantitative methods.

A striking illustration from a recent book by a prominent sociologist will make the point clear. After a discussion of the lamentable limitations of statistical methods, the author appends this remarkable footnote: "Wherever the statistical method definitely gains the ascendency, the number of students of a high intellectual level who are attracted to sociology tends to fall off considerably."[13]

In short, the author finally reverts to a statistical proof that statistics don't prove anything. It must be clear that the only operations as a result of which one could make the statement that, as statistical methods gain ascendency, high-caliber students decrease in numbers, would be (1) to measure the degrees of relative intelligence of students; (2) to measure the quantitative variations in registrations of the better students in different sociology departments; (3) to measure the degree to which quantitative methods dominate the

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[12] Funk and Wagnall's *New Standard Dictionary*, 1931.

[13] Florian Znaniecki, *The Method of Sociology*, N. Y., Farrar and Rinehart, Inc., 1934, p. 235.

departments; and (4) to correlate the last two factors. The statement is an excellent example of so-called non-quantitative techniques and suggests the reason for their popularity. The measurement of the factors here involved is a serious and difficult business. The generalization as quoted was the result of a few strokes of the pen. In short, what the critics of the better quantitative methods seem to prefer is informal, impressionistic, and imaginary statistics supporting their prejudices.

The surprising implication by other sociologists that Darwin did not use quantitative methods seems to rest on a somewhat similar misunderstanding.[14] The assumption seems to be that because the *Origin of Species* contains no tables, therefore Darwin did not use quantitative methods. The patient accumulation of thousands of cases, the painstaking classification of them, the recording of the proportion of cases supporting an hypothesis and the proportion contradicting it—all this apparently does not come within the definition of quantitative methods as understood by these writers. I am not here interested in entering into a dispute about the correctness of this definition or of my own, which is much broader. I am interested only in making the point that, if such limitations exist in the definition of quantitative methods on the part of those who find the methods of little value, I am not surprised at their conclusion. But it must be clear that, if the distinction between quantitative and non-quantitative techniques is to have any significance, we must take the position that a procedure is none the less quantitative or statistical if the operation is carried on without algebraic symbols or with concepts of *more* and *less* instead of with formal or exact numbers.[15] It may be alleged that this is an attempt to break down the distinction between the quantitative and the non-quantitative. That is precisely what I am interested in doing, so far as it is attempted to distinguish them solely on the basis of the formality with which they are carried out.

Objectification of the technique of generalization invariably results in quantification.

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[14] E.g., Herbert Blumer, *Amer. Jour. Sociol.*, Vol. 35, No. 6, 1930, p. 1102.

[15] "There can be no doubt," says Professor Morris R. Cohen, "that it is of the essence of scientific method that vague terms like *large* and *small*, *far* and *near*, *hot* and *cold*, etc., shall be replaced by terms made definite by measurement." M. R. Cohen, *Reason and Nature*, N. Y., Harcourt Brace and Co., 1931, p. 89. See also L. L. Bernard, "The Evolution of Social Consciousness and of the Social Sciences," *Psychological Review*, Mar. 1932; "The Development of Methods in Sociology," *The Monist*, April, 1928, pp. 292–320.

III

The above conclusion directly raises the question as to whether scientific generalization is always and necessarily quantitative. I contend that it is. Those who find otherwise must mean something different by the term *generalization*, and they have failed to explain in operational terms what they do mean by it. I mean by the verb *generalize* the process of determining the probable prevalence in a universe of a given datum or configuration of data. I mean by the noun *generalization* a statement arrived at by the above process. That is, I define the concept in terms of the operations by which I arrive at it, in conformity with the accepted requirement of science.[16] Is this or is it not what every scientist today means by generalization? If you accept this definition, the question as to whether all scientific generalization is necessarily quantitative at once disappears, for quantification is implicit in the definition. If you do not accept this definition, let us have your substitute. But let us have it in operational terms, i.e., in terms of the steps involved in arriving at it. If you cannot so define it, all argument as to its nature again

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[16] "Let anyone examine in operational terms any popular present-day discussion of religious or moral questions to realize the magnitude of the reformation awaiting us (p. 32). . . . I believe that many of the questions asked about social and philosophical subjects will be found to be meaningless when examined from the point of view of operations. It would doubtless conduce greatly to clarity of thought if the operational mode of thinking were adopted in all fields of inquiry as well as in the physical." P. W. Bridgman, *The Logic of Modern Physics*, N.Y., The Macmillan Co., 1932, p. 30.

"The only justification for our concepts is that they serve to represent the complex of our experiences; beyond this they have no legitimacy. I am convinced that philosophers have had a harmful effect upon the progress of scientific thinking in removing certain fundamental concepts from the domain of empiricism, where they are under our control, to the intangible heights of the *a priori*." A. Einstein, *The Meaning of Relativity*, Princeton University Press, 1923, p. 2.

The statement "no virtuosity of technique can compensate for want of understanding" (Waller [cited below] p. 290) uses the word "understanding" in precisely the way attacked by Einstein. Understanding *is* a "virtuosity of technique," from the operational point of view.

"The older elementalistic linguistic problems of 'matter,' 'space,' and 'time' were in such a mess, due to the objectification of verbal structures, that it was useless to talk any more in the old way. He [Einstein] decided to describe what a physicist *does* when he measures 'space' and 'time' and to abandon, perhaps unconsciously, the 'is' of identity." Korzybski, *op. cit.*, p. 648.

My restricted use of the term *science* in this section has been made the object of attack on the ground that "all science is a search for truth." The implied *non sequitur* is that therefore all search for truth is science. I have elsewhere fully recognized the value of pre-scientific, non-scientific, and non-quantitative techniques. (See G. A. Lundberg, "Is Sociology too Scientific?" *Sociologus*, Sept. 1933, pp. 301, 302, 316–317.) I use the term *science* to describe a method and its results, not to exalt that method or depreciate others. Also, I am dealing here only with the type of response known as generalization, i.e., the extrapolation of propositions from the known to the unknown. Such extrapolations may be made from single cases, but if so, there is no basis upon which to estimate the probable validity of such extrapolation.

disappears, as anything you say regarding your private mental operations must necessarily be accepted as final and not subject to check, and therefore outside the pale of science.

As a matter of fact, I think the definition of generalization I have given above is what everyone means by the term. Those who fail to recognize it as such are simply misled by the informality with which the process is carried out, as I have illustrated above. The delusion that a scientific generalization may be drawn from a single case seems to be due to the fact that sometimes a single case happens to illustrate, typify, or coincide with the facts as stated in a generalization. This is apparently at the root of such a statement as appeared recently in a leading article of the *American Journal of Sociology*. "If one perceives a single case correctly," says the author, "he can generalize from that instance."[17] We are left without any operational clue as to how to perceive correctly or how to determine the correctness of a perception. Actually, of course, we say a perception is "correct" when other qualified observers confirm our report on an observation. But even when this agreement of perceptions of a single case has been established, on what possible logical grounds may one postulate that the datum or configuration of data is present more generally in the universe? There are no grounds whatever for such an assumption in the absence of further observations of additional instances.

The use of the word "correctly" above is a striking illustration of how terms of this kind frustrate thinking in the social sciences. The author clearly uses it to *mean* an observation which (1) *has been* confirmed by other qualified observers and (2) *would be* found to hold for the whole universe under consideration. In short, he jumps over the operational steps implied in the words "perceiving correctly" and thus reaches the conclusion that there are no such describable steps, but that the conclusion is "directly" revealed through the alchemy of the mind. It is this kind of verbal necromancy which has

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[17] W. Waller, "Insight and Scientific Method," *Amer. Jour. Sociol.*, Vol. 40, No. 3, November 1934, p. 287. I refer to this article chiefly because it furnishes illustrations from recent literature of the points I wish to make. It should not be inferred, therefore, that this article is especially hostile to quantitative methods, but rather the contrary. Since much of the controversial literature in sociology consists of heroic demolitions of positions nobody holds or defends, I have tried to avoid this time-honored technique of scholastic discussion in the present paper by citing in each case proponents of the views I attack. For other illustrations see J. F. Brown, "Towards a Theory of Social Dynamics," *Jour. Soc. Psych.*, Vol. 6, 1935, pp. 188–189; and K. Lewin, *A Dynamic Theory of Personality*, N. Y., McGraw-Hill Book Co., 1935, pp. 12, 14, 31.

compelled science to insist on the operational definition of its concepts. Of course, I distinguish between what I have defined as a scientific generalization and an hypothesis, although they differ only with respect to the adequacy of the data on which they rest. Thus, the statement criticized above and such contentions as Brown's that laws precede their demonstration confuse hypothesis with scientific law.[18]

While the author quoted above avoids giving any instructions as to how to perceive with that "correctness" which will permit us to generalize from a single case, he does essay directions as to how "we may proceed to obtain insight," and Karl Pearson himself could not improve upon them. For here it is set down categorically that "*in order to perceive with insight*," we must engage in (1) "direct study of human and interhuman behavior," (2) study of symbols supposed to stand for such behavior, and (3) "sympathetic penetration."[19] In short, insight is not itself the method or even the beginning of the process but the result of some very mundane procedures many of the details of which in their more refined and systematic form may be found in any good text on statistics. This is what I mean by an operational definition of insight as compared with such exhortations as "try to see how data arrange themselves"; "experience phenomena with insight"; "we must look at events until they become luminous."[20] For commentary on these verbal gyrations, it would be impossible to improve on the same author's own remarks two pages later on some other matter. "An unfortunate circumstance," he says, "is that communication often breaks down, so that one acquires names without their attendant perceptual patterns. There is abundant evidence in sociological literature that many of our colleagues have learned words without perceiving processes, so that they literally do not know what they are talking about."[21] To which I append a fervent "Amen!"

I conclude that the notion that a scientific generalization can be drawn from a single case arises from a failure to define concepts in operational terms. Further illustration of the same confusion is found in certain current discussions of causation. Thus Köhler says: "Once more I must point out that our feeling of something naturally

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[18] J. F. Brown, "A Methodological Consideration of the Problem of Psychometrics," *Erkenntnis*, 1934, No. 4, pp. 46–61.

[19] W. Waller, *op. cit.*, p. 288.

[20] *Ibid.*, pp. 287, 288.

[21] *Ibid.*, p. 290.

dependent upon something else does not refer to a correlation, or a highly constant togetherness *as such*, stated in terms of the external observations of a great many cases. It refers rather to an evident dynamical dependence as experienced *hic et nunc*, in one actual case."[22] The "*feeling* of something *naturally* dependent" and the "*evident* dynamic dependence" of two things as "felt" by an individual in "one actual case" is precisely what has led to some of the most preposterous generalizations in history. The fact that at other times such "feelings" from a single case have later been confirmed and found to hold generally in no way justifies us in confusing the hypothesis with the verified generalization. The latter is exactly what distinguishes science from other types of knowledge. Nor is this distinction any less clear or important because hypotheses are a proper part of the scientific method.

The above quotation from Köhler is an example of one of the postulates of Gestalt psychology. It seems to be the dismal destiny of sociology to fight the battles of psychology over again a decade or so after the issue has been settled or abandoned in the latter science. Thus, some of the questions regarding behaviorism which had their inning twenty years ago in psychology are still in the foreground of sociological discussion. It is not surprising, therefore, that sociologists have now discovered Gestaltism and with it they are hoping to stave off both behaviorism and quantitative methods. In the meantime alas, the Gestalters in psychology (especially the younger workers in the field) declare Gestaltism to be a form of behaviorism and frankly avow their adherence to quantitative and mathematical techniques.[23] Thus Koffka says in his recent book: "In my opinion this famous antithesis of quantity and quality is not a true antithesis at all. It owes its popularity largely to a regrettable ignorance of the essence of quantity as used in physical science (p. 13) . . . the quantitative, mathematical description of physical science, far from being opposed to quality, is but a particularly accurate way of representing quality (p. 14) . . . It (psychology) may be perfectly quantitative without losing its character as a qualitative science, and on the other hand . . . it may be unblushingly qualitative, knowing that *if its*

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[22] W. Köhler, *Gestalt Psychology*, N.Y., H. Liveright, 1929, p. 361. Waller takes a similar position, *op. cit.*, pp. 285–90. Regarding the notion of "immediate experience" see E. C. Tolman, "Psychology versus Immediate Experience," *Philosophy of Science*, July 1935, pp. 356–380.

[23] See J. F. Brown, *The Mathematical Conceptions Underlying the Theory of Psychological and Social Fields*, Ann Arbor, Edwards Bros., Inc., 1935.

qualitative descriptions are correct, it will sometime be possible to translate them into quantitative terms (p. 15)."[24] (Italics mine.) This concedes, of course, the point I have made above as to the quantitative test of the correctness of a generalization.

Even more striking is the declaration of J. F. Brown. "It may well be," he says, "in fact I think it very likely, that at best psychological analysis may only be statistical."[25]

Yet in the face of these declarations by accredited Gestaltists a recent article in the *American Journal of Sociology* purporting to be "some methodological implications of the *Gestalt* principle of insight" has this passage: "The relationship of cause and effect usually assumes the form of a configuration in time. This theory enables us to avoid the ultimate nonsense of Pearsonian methodology, the doctrine that a statement of a causal relation is really only a statement of relative probabilities."[26] As to whether *cause, as science understands it*, is an "elementary datum of experience" which has nothing to do with relative probabilities I am content to refer you to a bibliography on the subject including such names as Bohr,[27] Bertalanffy,[28] Bridgman,[29] Heisenberg,[30] Hecht,[31] and Schroedinger[32] in science, and others of equal repute in philosophy.[33] If the author of the above quotation will provide you with an equally adequate bibliography of the other view, you may study the two positions at your leisure

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[24] K. Koffka, *Principles of Gestalt Psychology*, N.Y., Harcourt Brace and Co., 1935. In a footnote Koffka attributes a similar idea to Wertheimer.

[25] Brown, *op. cit.*, p. 4.

[26] Waller, *op. cit.*, p. 287.

[27] N. Bohr, *Atomic Theory and the Description of Nature*, N.Y., The Macmillan Co., 1934, Chap. IV, pp. 106, 109, 110.

[28] L. Bertalanffy, "Über die Bedeutungen der Umwälzungen in der Physik für die Biologie," *Biologisches Zentralblatt*, Vol. 47, 1927, pp. 653–662. See also on this subject Hans Müller, *et al.*, *Cold Spring Harbor Symposia on Quantitative Biology*, Vol. II, Biological Laboratory, Cold Spring Harbor, N.Y., 1934.

[29] P. W. Bridgman, *The Logic of Modern Physics*, p. 37: "I believe that examination will show that the essence of an explanation consists in reducing a situation to elements with which we are so familiar that we accept them as a matter of course so that our curiosity rests. Reducing a situation to elements means, from the operational point of view, discovering familiar correlations between them."

[30] W. Heisenberg, *The Physical Principles of the Quantum Theory*, University of Chicago Press, 1930, Chap. IV, pp. 58–59, 63.

[31] S. Hecht, "The Uncertainty Principle and Human Behavior," *Harper's Magazine*, January 1935, pp. 237–249.

[32] E. Schroedinger, *Science and the Human Temperament*, N.Y., W. W. Norton Co., 1935, Chaps. II, III, and V, especially pp. 59, 64, 66, 131–32.

[33] B. Russell, *The Analysis of Matter*, N.Y., Harcourt, Brace and Co., 1927, Chaps. XVI, XX, XXX, XXXI, XXXV, XXXVIII.

J. Dewey, *The Quest for Certainty*, London, Allen and Unwin, 1930, pp. 28, 191–192, 194, 198, 276.

and decide the matter for yourself if you are still interested. There is no question whatsoever as to what living and working scientists today mean by the word "cause," in so far as they concern themselves with it.[34] They have defined it in terms of the operations by which they arrive at it.[35]

IV

I have criticized above the interpretation which some sociologists have made of Gestalt theory as bearing upon the importance of quantitative methods in sociology. I believe that the position I have criticized is not only invalid in itself but that it is a misinterpretation of the true position of Gestalt theorists, especially of the younger workers in the field. It is a pleasure in this connection to recognize the importance of the Gestaltists' contribution in their theory of fields and topology.[36] The theory of fields is an attempt to provide a more adequate frame of reference for psychological and social dynamics. By dynamics we understand that aspect of science which deals with systems undergoing change. Implicit in dynamics is the idea of force. Behavior in all science is assumed to take place in space. The basic postulates of behavior are accordingly a force (or forces) and a field within which forces operate.[37] When there is a difference in potential within a field, change or behavior occurs. In the symbolic description of this behavior situation, especially in its earlier or theoretical stages, geometry has been found most useful. In fact, geometry is in modern times perhaps the chief mathematical tool for the construction of scientific theory.[38] The special branch of geometry developed

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[34] L. K. Frank, "Causation: An Episode in the History of Thought," *The Journal of Philosophy*, Vol. 31, No. 16, August 2, 1934, pp. 421–428.

[35] Operationally speaking, cause is imputed to the independent variable or combination of variables when it shows a high probability-expectation in its concomitant variations with other factors or combinations of factors, other supposedly relevant conditions held constant.

[36] J. F. Brown, *op. cit.*, p. 7. See also Philip Franklin, "What is Topology?," *Philosophy of Science*, January 1935, pp. 39–47.

[37] "All dynamical theory tends to be field theory. The whole trend of modern science is away from theories which consider the behavior of objects determined by the class to which they belong, to theories which consider the behavior of objects to be determined by the field structure." Brown, *op. cit.*, p. 10. I have taken a similar position in my paper "Public Opinion from a Behavioristic Viewpoint," *Amer. Jour. Sociol.*, Vol. 36, Nov. 1930, pp. 387–405. I am unable to see any conflict or contradiction between the above position and contemporary behaviorism. Behaviorists recognize as fully as any group the configurational nature of social situations. Their alleged atomistic methods are merely a recognition of the necessity to reduce a complex problem to manageable proportions and to solve one problem at a time. For further discussion of this point see G. A. Lundberg, "Is Sociology Too Scientific?," *Sociologus*, Vol. 9, No. 3, September 1933, p. 307, *et seq.*

[38] *Cf.*, Brown, *op. cit.*, p. 5. There is, however, an important difference of opinion on this subject among scientists and mathematicians.

to serve this need is topology, which deals with the non-metrical aspects of space, such as relationships of connection and position.[39] Forces directing movement may be represented by vectors.[40] "The strength and direction of these vectors is determined by the distribution of the objects in space and not within the object . . . The parts cannot be separately considered as activating forces but what happens at any individual point within the field is a function of the whole field structure."[41] "Since rational thinking tends to structure itself in terms of special relationships, topology gives us the mathematics necessary to set up theories about psychological and sociological problems where fundamental measurement is impossible at the present time."[42] Thus the concept of social fields and forces geometrically representing social situations may be of the greatest importance as the theoretical framework within which meaningful quantitative description of processes may be carried out. The ultimate goal is to describe these processes in functional equations.

The significance of this approach in sociology has been most adequately illustrated by Chapin.[43] He has shown how the graphic symbolic representation of structural configurations succeeds in breaking down a whole into measurable parts, in identifying the part-whole relationships, and in helping to visualize the subtler patterns of intangible relations. The refined analysis of the configurational elements rests upon the development of quantitative indices of behavior for which both verbal and graphic symbols ultimately stand. Here again Chapin has pointed the way by defining opera-

[39] *Cf.*, Brown, *op. cit.*, p. 7.

[40] "The tensor calculus is an extension of the vector calculus, which has become famous since Einstein. It gives us formulations independent of any special frame of reference. In using it we are automatically prevented from ascribing to the events around us characteristics which do not belong to them. The tensor equations give us absolute formulations, absolute being understood as relative no matter to what." A. Korzybski, *Science and Sanity*, p. 619.

"The essence of Einstein's generalization is its final disentanglement of that part of any physical event which is contributed by the observer from that which is inherent in the nature of things and independent of all observers." E. T. Bell, *The Principle of General Relativity* (quoted in Korzybski, *op. cit.* p. 635).

[41] Brown, *op. cit.*, p. 10. This does not change the fact that the original definition of the field is a construct of convenience *relevant to and determined by the problem pursued*, rather than a definition inherent in the universe, as is implied in much philosophical discussion of the whole-part relationship. E.g., Korzybski, *op. cit.*, p. 91; Waller, *op. cit.*, p. 287; Lewin, *op. cit.*, pp. 21, 31. In a forthcoming paper I shall deal in greater detail with some of the subtler fallacies of Lewin's plausible and in many respects valid analysis. Basic questions of language and knowledge are involved, for a general treatment of which see A. F. Bentley, *Behavior, Knowledge, Fact*, Part II. Bloomington, Ind., Principia Press, Inc., 1935.

[42] Brown, *op. cit.*, pp. 7, 8.

[43] F. S. Chapin, *Contemporary American Institutions*, N.Y., Harper and Bros., 1935, Chap. 16.

tionally the concept of social status by a three-dimensional vector. A good deal of the quantitative work that has been done in sociology and social psychology, however unorganized and uncoördinated with reference to basic theory, represents a movement toward this end. In addition to the work of Chapin, special attention must be called to the notable work of Thurstone,[44] Woolston,[45] and Dodd[46] in the development of vectorial techniques, Kirkpatrick's[47] approach to the problem of attitude configurations, and the studies of Moreno,[48] and Thomas, Loomis and Arrington[49] of fundamental group dynamics. These methods represent pioneer attempts to metricize data and fields which have up to the present been regarded as least amenable to quantitative methods.

It is my considered judgment that these very recent contributions by the authors mentioned represent most significant methodological developments in the field of social psychology. A full understanding and appreciation of them requires a careful study of the sources cited, where the techniques and the underlying theory are fully set forth. An adequate review of a single one of these studies would be beyond the space limits of the present paper. Furthermore, it has not been my purpose in this paper to discuss technical details or to catalog the large number of significant quantitative contributions to social psychology. Such reviews have recently been published, and I refer you to them.[50] I have preferred to confine myself instead to what I consider the crucial issues regarding quantitative techniques.

While the studies which I have just now estimated so highly are too recent to have received much discussion as yet, I should in con-

[44] L. L. Thurstone, *The Vectors of Mind*, University of Chicago Press, 1935. See also under the same title his article in the *Psychological Review*, January 1934. pp. 1–32.

[45] H. Woolston, "American Intellectuals and Social Reform," forthcoming in *American Sociological Review*, June, 1936; "Discrimination: A Study of Social Determinants," *Journal of Social Psychology*, Vol. 5, 1934, pp. 248–254. See also his article "Stepbrothers," *Social Forces*, Vol. 6, 1928, pp. 368–375.

[46] S. C. Dodd, "A Theory for the Measurement of Some Social Forces," *Scientific Monthly* (forthcoming). Also his "*A Controlled Experiment in Rural Hygiene in Syria*," American University of Beirut, 1934, Part IV. See also on this subject: N. Rashevsky, "Outline of a Mathematical Theory of Human Relations," *Philosophy of Science*, October 1935, pp. 413–430.

[47] C. Kirkpatrick, "Attitude Measurement and the Comparison of Generations," *Jour. App. Psych.* (forthcoming).

[48] J. L. Moreno, *Who Shall Survive? A New Approach to the Problem of Human Interrelations*, Washington, D. C., Nervous and Mental Disease Publishing Co., 1934.

[49] D. S. Thomas, A. M. Loomis, and R. E. Arrington, *Observational Studies of Social Behavior*, Yale University, Institute of Human Relations, 1933.

[50] *Handbook of Social Psychology*, Ed. by C. Murchison, Clark University Press, 1935. See especially Chap. XVII, "Attitudes," by G. W. Allport; also G. and L. Murphy, *Experimental Social Psychology*, N.Y., Harper and Bros., 1931.

clusion like to anticipate the two principal objections which they are sure to arouse: First, they represent a departure from the customary terminology and concepts of sociology and will therefore incur the hostility with which everything new and strange is met, especially when it threatens the *status quo*. Secondly, these new methods, as a result of their novelty, do not lend themselves at the present time to the kind of ready verification through common sense upon which even scholars are inclined to insist in the social sciences. Both objections are really correlative and can be considered together.

The literature of sociology has been until recent years couched almost exclusively in the literary style of oratory, homiletics, and the novel. Any departure from this style is likely to cause suspicion and resentment. The point is best illustrated from current reviews of some of the better sociological monographs. "The author has leaned over backward," says one reviewer, "in his attempt to be scientific and *as a result* his presentation leaves much to be desired"[51] (italics mine). The implication is that in proportion as a sociological monograph lapses into scientific style or terminology it is objectionable to sociologists regardless of what its merits may be. There is no criticism more common in sociological reviews than that the research under review lacks "the human touch," "one encounters no real human beings in these pages," "the pages do not 'live'," etc., etc. Now we all like it if our work turns out to be also entertaining. I, too, appreciate the sadistic and the Freudian satisfactions which the literature of sociology provides for large numbers within and outside the field. Nor am I defending obscurity, pedantry, or scientific camouflage. I merely venture to question whether conformity to antique style, concepts, and logic are legitimate criteria for the evaluation of sociological contributions. I appreciate as highly as anyone the sociological novel, poetry, and the movies. But to confuse them with scientific contributions seems to me to be a step to which both scientists and novelists may properly object.

Underlying the above confusion is the appeal to common sense, which in spite of its history and present status in the other sciences, still has tremendous prestige in sociology. Sociologists have overlooked that the only legitimate regard to which common sense is entitled is that which it may deserve when contrasted to no sense at all, and not as contrasted to the uncommon sense which is science. The place of common sense in the other sciences has been well de-

[51] Review of S. C. Dodd, *op. cit.*, in *Sociol. and Soc. Res.*, Sept.-Oct. 1935, p. 86.

scribed by Bell: "That is precisely what common sense is for," he says, "to be jarred into uncommon sense. One of the chief services which mathematics has rendered to the human race is to put common sense where it belongs, on the topmost shelf next to the dusty canister labeled 'discarded nonsense'."[52]

In short, we cannot accept as valid criteria of the scientific value of sociological research its power to entertain or the degree to which it coincides with the stereotypes we have inherited from primitive folklore, or which we have formed from contemporary journalism and cartoons. There has been some consternation in recent years over the fact that the anthropologist's Indians do not resemble very closely the Indians found in the books of James Fenimore Cooper and Longfellow. Shall we therefore set aside the anthropologist's findings on the ground that they are less "real," "live," "human" and true to "common sense" than our stereotypes of Indian culture drawn from novels and poetry? Germany today provides a fine example of what the glorification of folklore in preference to scientific anthropology will do for a nation. Sociologists still have to decide apparently as to whether they prefer the pronouncements of the cracker-box philosopher or the graphs of Ogburn and his collaborators as to what are social facts and trends. The techniques by which conclusions in social psychology will be reached will, I predict, become increasingly strange to the common man. Sociologists will have to decide whether on this ground alone they wish to set aside the increasing body of quantitative sociological knowledge which flatly contradicts the conclusions of "common sense." In the meantime we shall no doubt have increasing lamentation over the necessity for this choice. For as Santayana has said: "The philosophy of the common man is an old wife that gives him no pleasure, yet he cannot live without her, and resents any aspersions that strangers may cast on her character."[53]

Let us by all means have bigger and better understanding, intuition, and insight. One way to get it is for those who have it to let the rest of us in on the technique. Also, unless it is communicated the technique will die with its present possessors and posterity will be bereft of it. Let us likewise by all means study the widest and most intricate configurations and Gestalten. But let us not deceive our-

[52] E. T. Bell, *The Queen of the Sciences*, Baltimore, Williams and Wilkins, 1931, p. 16.
[53] G. Santayana, *Scepticism and Animal Faith*, N.Y., Scribners, 1923, p. 11.

selves that by the mere grandeur of the undertaking we can avoid rendering account of our methods in operational terms.

The transition to quantitative methods in the social sciences is increasingly evident. The Econometric Society is firmly established. A Psychometric Society has been organized within the last year. A Sociometric group already meets informally within the Sociological Society. These developments are only the first reverberations in the social sciences of the main intellectual battle of the present generation, namely, the battle against the shackles of Aristotelian verbalisms, logic, and laws of thought.[54] The simple fact is that the conceptual tools of primitive folklore are totally inadequate for a coherent orientation in the new world which science has revealed to us. We cannot deal with the physical world with the tools of 1935 and at the same time confront social problems with the tools of the Middle Ages, or of 5000 B.C., without the most serious schizophrenic consequences to social behavior.[55] The pronounced trend of the last ten years toward quantitative and mathematical techniques in sociology is only a prelude to the transition in that direction which is impending.

DISCUSSION

WILLARD WALLER
Pennsylvania State College

I AM SURE that we are all profoundly grateful to Dr. Lundberg for his brilliant statement of some of the implications of the doctrine of operationalism as applied to our field. Operationalism is a new but respectable doctrine which has been contributed to modern philosophy by the physicist, Bridgman. In order to cope with the contradictions which the newer doctrines had introduced into the field of physics, Bridgman proposed that the procedures and assumptions upon which a concept was based should in each case be made a part of the concept. The principal concern of operationalism is with measurement. The operationalist procedure is useful, but we should not think that it excludes all other procedures. As Benjamin has remarked: "It must not be supposed that the operational definition is to *replace* the formal definition, however valuable it may be as a

[54] O. L. Reiser, "Non-Aristotelian Logics," *The Monist*, January 1935, pp. 100–117.

[55] *Cf.* Korzybski, *op. cit.*, p. 149. See also Read Bain, "Our Schizoid Culture," *Sociol. and Soc. Res.*, Vol. 19, Jan.-Feb. 1935, pp. 266–276.

supplement. They are essentially different in function. The formal definition is qualitative; it enables us to identify an instance of a given concept. The operational definition is not a statement of the qualitative nature of the concept under consideration, but rather a description of the processes involved in determining its measure. i.e., its quantitative correlate. . . . Measurement is no doubt important, but it is well to know just what it is that we are measuring."[1] As I see it, it does not matter at the present time whether quality will sometime be resolved into quantity or not. Disputes resting upon the assumption of perfect knowledge are less important to us than the question what we should do with imperfect knowledge, and in the present stage of our knowledge the quality-quantity antithesis still retains some significance.

Dr. Lundberg's use of this contribution from the field of physics raises the question of the advisability of importing the methodology of the physical sciences into social science. The physical scientists have progressed further than we have, and we are in a position to learn from them. One of the most important lessons the physical scientists can teach us is that method must always be flexibly adapted to subject matter. As data change, procedures and assumptions must change. The physical scientist will be the first to chide us for making his tentative pronouncements into hard and fast rules and applying them in a different field. No one questions that physical science methods can be employed in social science to a certain extent, but it is also clear that literalism in the application of physical science rules can lead only to a parody of science. It is my notion that our knowledge of human beings is nearly always mixed, being composed in part of external, behavioristic observations and in part of an inner, imaginative knowledge, so that it will probably be necessary for social science to modify physical science methods considerably.[2] But much remains to be done before we can be at all certain just what method or combination of methods is likely to be useful. Until we know a great deal more than we do now, we ought to avoid a rigid, aprioristic limitation of our field and methods, and I must add that it seems to me that Dr. Lundberg's paper tends in that direction.

To take up a few specific criticisms, I feel that Dr. Lundberg might

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[1] A. C. Benjamin, Review of P. W. Bridgman, "*The Logic of Modern Physics*," *Journal of Philosophy*, Vol. 24, pp. 663–665.

[2] I have attempted to differentiate and show the interrelations of different kinds of social knowledge in a paper entitled "Insight and Scientific Method": *Amer. Jour. Sociol.*, Vol. 40, Nov. 1934, 285–297.

have expressed himself more clearly in certain portions of his paper. I had particular difficulty in making out his position on the following points:

1. How does he use the word insight? On pp. 40–41 he says, "The error lies in overlooking that insight and understanding are the ends at which all methods aim, rather than methods in themselves." And on p. 40 we read, "They overlook that the understanding, reason, and logic which they properly advocate is itself a method, a technique, of some kind." And, in a footnote on p. 44 we are told, "But understanding is a virtuosity of technique." Later we gather that insight, understanding, etc. may be viewed as a method or an end, but Dr. Lundberg does not offer an operational description of the techniques involved. I do not suggest that anything more than apparent contradiction caused by unfortunate use of language is involved here. What worries me is that some unfriendly critic may conclude that this apparent self-contradiction betrays the absence of that specificity of denotation which is so widely regarded as desirable in scientific writing. There were other examples of this sort of self-contradiction in the paper.

2. Just how much does the case for the informal statistical method depend upon "word-magic"? You will recall the following passage: "In fact, the insight and understanding which they seek is to be achieved only by further correlation—formal or informal. Correlation is not merely the name of a certain statistical operation invented by Karl Pearson. It is, as the dictionary says, 'the act of bringing under relations of union or interaction.' As such, it is a method used as frequently by other people as by statisticians." This other meaning of the word, *correlation*, which is not very clearly quantitative in its denotation, seems to have been introduced for the purpose of making a play on words. The underlying formula is this: Thinking is correlation, and therefore *correlation*, and therefore quantitative. Otherwise, what could be established by showing that the word *correlation* has another meaning? What does it matter how many meanings the word has or what they are? It is certainly very unusual to derive scientific methods from dictionary definitions. It is perfectly legitimate to introduce variant definitions in order to establish and maintain a distinction. That is one means of refining concepts. Here we are faced with the opposite procedure. A secondary meaning is established with a view to showing that the second thing is the same as the first, because they are called by the same

name. The result is that at many points in the paper one cannot tell what kind of correlation Dr. Lundberg is talking about.

The verbal confusion noted above was wholly unnecessary. I do not believe that Dr. Lundberg bases his case upon it to any considerable extent. There are plenty of instances of informal statistical method, and I am sure that he had those in mind when discussing the category. I believe that there are also mental operations of a scientific nature for which this is not an apt description. Possibly he has fallen into this unfortunate use of language in the effort to reduce these exceptional cases to his formula.

I skip over several other criticisms of the sort, which I regard as secondary, if not trivial, in order to come more quickly to my principal point. For I must confess that I believe that the paper was in the wrong vein entirely and completely off the subject.

I agree heartily with Dr. Lundberg's statement concerning the real harmony of methods. And yet it did not seem to me that his paper was exactly conciliatory in tone or that it would bring the practitioners of different methods closer together. Disagreement between statisticians and practitioners of the so-called insight methods is by no means inevitable. It seems to arise in large part from the fact that one group concentrates attention upon procedures partly extra-mental, upon the manipulation of external symbols, whereas the other group is interested in internal behavior. There is a basis for this contrast in Dr. Lundberg's paper. These procedures are different aspects of the same thing, and cannot even be sharply contrasted. Naturally, in developing our methodology, we must pay attention to all phases of the knowing process.

I believe that we might very well have expected of Dr. Lundberg a paper of a different kind. The best and most convincing argument in favor of any method is to show how it may be used productively. It will be a great service to us all, if someone will think through the problems involved in quantifying the difficult field of social psychology, and make suggestions as to procedures. A promising beginning has been made, but there are still problems which must be faced and solved.

We must remember that the movement of thought known as social psychology has been stimulated from the first by a discontent with the results of the more formal methods of physiological psychology. Social psychology as it exists today comprises a very broad and general attack upon problems of human nature which are not

adequately treated elsewhere. Probably its methods must continue to be varied, but I feel sure that as quantitative and experimental methods are developed they will attain a ready reception.

A good many different kinds of knowledge are comprised in the field of social psychology, and they are amenable to statistical treatment in varying degrees. Some fragments of the field have already been subjected to statistical investigation, others to statistical statement, and a respectable body of experimental knowledge has also grown up. Other knowledge may adequately be described as informally statistical. For other bits of knowledge this description does not seem suitable to me; I somehow cannot think of Mead's analysis of communication or of Cooley's looking-glass self in quite those terms. One may say that the Mead-Cooley type of thing is not science. I confess that I have been granted no special revelation as to the proper use of the word *science*, and I am willing to conform to any established usage. If the illuminations of Mead and Cooley are not science, they are at least important knowledge concerning human nature, and they must somehow be dealt with by any science which is to penetrate deeply into human life.

The view which I should like to see established is that artistic and quantitative methods are interlinked in such a way that progress in the one necessarily involves corresponding progress in the other.[3] There is no real conflict, but an inter-dependence between these methods. The association of quantitative study in social psychology with the narrower sort of behaviorism has been unfortunate, largely

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[3] The term "artistic," which has been used by James W. Woodard, seems somewhat more accurate than other phrases such as "insight methods." Woodard also stated essentially the view of the interrelations of method which we give here in a paper entitled, "Interrelations between the Quantitative and Non-quantitative Methods," which was presented to the Eastern Sociological Society in 1933, but has not yet been published. A brief published statement of the Woodard point of view is to be found in James W. Woodard, "Five Levels of Description of Social-Psychological Phenomena," *Sociologus*, Vol. 9, 1933, 4–10. Karpf has expressed a similar view in the following passage: "The same, in substance, may be said in regard to the question of method in social psychology. Here, again, it is fruitless to argue abstractly that either one or the other of the established scientific methods is best adapted to social-psychological investigations. So far, as a matter of fact, experiment, systematic observation, testing and rating techniques, case and monographic study, the statistical method, the historico-genetic and ethnological methods have all been successfully used in the field, and seemingly each is supplementary to the others. Experiment is needed, for instance, for verification; case and monographic study, for the investigation of processes and the setting up of causal hypotheses; statistics, for the establishment of norms; etc. Only the further application of these various methods in the investigation of concrete problems can determine more clearly than appears now what are the most effective lines of cooperation between them in so far as the field of social psychology is concerned. F. B. Karpf, *American Social Psychology, Its Origins, Development, and European Background*, p. 427.

because it has obscured this connection. While fervently denouncing introspection, early workers had to depend largely upon introspective data as gathered from a schedule or a questionnaire; and yet because introspection was a bad method anyhow these workers refused to learn how to use it effectively. It is now recognized that a certain amount of artistry in framing and arranging questions on a schedule, in eliciting responses, and in interpreting data greatly enhances the value of such studies. Questions should elicit real opinions, not pretended or merely theoretical or non-existent opinions. Where possible, they should call out opinions that are important in one's scheme of life. Responses should not be stereotyped too much; here the case study or the life history is an admirable corrective. Where possible, a statement as to behavior in an actual situation should be called for. In many studies, artistic technique in interviewing is desirable. Interpretation is always artistic and imaginative on one level or another.

A few sociologists have been able to study behavior quantitatively, very often with valuable results. The difficulty here is that there are different levels of interpretation of human behavior, and the artistic element involved in the recognition of the unit increases as one progresses to those interpretations which give meaning to the largest number of facts; Woodard analyzed this very ably a few years ago.[4] Progress in artistic methods must here keep pace with quantification.

The degradation of concepts presents a real problem. In stating a concept statistically, changes are made in it in order to make measurement possible. Sometimes these changes are improvements. In other cases a concept is so simplified as to be almost unrecognizable. I am afraid some of the famous studies on social distance illustrate this mechanism rather well.

Some writers are very confident that they are able actually to measure attitudes. As Kirkpatrick has recently pointed out, these measurements are in fact classifications, and rest upon certain assumptions which deserve to be recognized.[5]

A good deal has been said about the use of case studies, life histories, and other materials relating to personality in quantitative treatments. There are great possibilities here, but the matter re-

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[4] *Op. cit.*

[5] Clifford Kirkpatrick and Sarah Stone, "Attitude Measurement and the Comparison of Generations," *Jour. App. Psych.*, Vol. 29, pp. 564–582, October, 1935.

quires study. It is necessary to define and identify significant units and to get some uniformity of records without destroying the essential value of the case study.

In solving these problems, it seems to me that the best results will be attained by a combination of techniques. For the present, the best general procedure for the quantitative worker will possibly be to work in those fields which are already informally statistical. In other fields, numerous exploratory studies are probably necessary before any effective quantification can be attained. In numerous instances, quantification will come first. I should like to suggest that no one method or set of procedures in the field will be stronger because the others are weak.

A CRITICAL STUDY OF THE CRITERION OF INTERNAL CONSISTENCY IN PERSONALITY SCALE CONSTRUCTION

RAYMOND F. SLETTO
University of Minnesota

A PERSONALITY scale in its developmental stage usually contains some items that are not sufficiently discriminating to justify their retention in the final form of the scale. The criterion of internal consistency is frequently utilized to select the more discriminative items. It is essentially the use of total score on all items of a scale to evaluate each individual item.

In the application of this criterion all items are scored according to uniform arbitrary scoring weights, and these total scores are placed in an array. Next, papers are segregated for persons whose total scores place them in the extremes of the array. The proportion included in these extreme segments has varied widely from one study to another; deciles, quartiles, quintiles and other proportions have been employed. Whatever the proportion used, the segments drawn from each extreme of the array are equal in the number of persons they contain.

Item responses are tabulated separately for the two extreme segments. The extent to which an item yields response differentiating between them is the measure of its discriminative value in the scale. The statistical significance of this discriminative value is usually determined by computing the critical ratio.[1]

The criterion of internal consistency was recently employed to select items for a battery of six scales developed by E. A. Rundquist and R. F. Sletto.[2] These scales were designed to measure morale, feelings of inferiority, family adjustment, attitude toward law, economic conservatism, and attitude toward education. Each scale contained 22 items, and five alternative responses were provided for each item. Typical items were as follows:

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[1] For a discussion of other methods of applying the criterion, see Joseph Zubin, "The Method of Internal Consistency for Selecting Test Items," *Jour. of Educ. Psych.*, Vol. 25, May 1934, 345–356.

[2] A monograph describing construction of these scales is near completion.

Morale	THE FUTURE LOOKS VERY BLACK.				
	Strongly[5] agree	Agree[4]	Undecided[3]	Disagree[2]	Strongly[1] disagree
Inferiority	IT IS EASY TO HAVE A GOOD TIME AT A PARTY.				
	Strongly[1] agree	Agree[2]	Undecided[3]	Disagree[4]	Strongly[5] disagree
Family	MEMBERS OF THE FAMILY ARE TOO CURIOUS ABOUT ONE'S PERSONAL AFFAIRS.				
	Strongly[5] agree	Agree[4]	Undecided[3]	Disagree[2]	Strongly[1] disagree
Law	PERSONAL CIRCUMSTANCES SHOULD NEVER BE CONSIDERED AN EXCUSE FOR LAW-BREAKING.				
	Strongly[1] agree	Agree[2]	Undecided[3]	Disagree[4]	Strongly[5] disagree
Economic Conservatism	THE GOVERNMENT SHOULD TAKE OVER ALL LARGE INDUSTRIES.				
	Strongly[5] agree	Agree[4]	Undecided[3]	Disagree[2]	Strongly[1] disagree
Education	OUR SCHOOLS ENCOURAGE AN INDIVIDUAL TO THINK FOR HIMSELF.				
	Strongly[1] agree	Agree[2]	Undecided[3]	Disagree[4]	Strongly[5] disagree

The reliability coefficients for these scales have usually been above .80 for various groups given the scales.[3]

The five alternative responses ranging from *strongly agree* to *strongly disagree* were taken from O. Milton Hall[4] who utilized them in a scale to measure occupational morale. These five alternative responses to each item were scored by means of arbitrary weights ranging from 1 to 5. One-half of the items were so worded that the *strongly agree* response appeared most favorable, and an equal number were so worded that the *strongly disagree* response appeared most favorable. The response judged to be most favorable was consistently assigned the lowest scoring weight.

Table I illustrates how item discriminative values were computed by the method of internal consistency.

It indicates how persons scoring in the highest and lowest quartiles on the morale scale responded to the statement: "The future looks very black." At the left are the five alternative responses ranging from *strongly agree* to *strongly disagree*. The first column contains

[3] Sixty-one of seventy-eight coefficients computed for 13 groups were above .80.

[4] O. Milton Hall, "Attitudes and Unemployment," *Arch. of Psych.*, 1934, No. 165, pp. 65.

the scoring weights assigned to the various responses. Column 2 shows the number responding in each position for the high total score quartile. Column 3 contains the products of the frequencies multiplied by the scoring weights. Columns 4 and 5 contain similar statistics for the low total score quartile. Summing the frequencies multiplied by the scoring weights yields a total of 83 scoring points for those in the highest quartile, and 49 for those in the lowest. Dividing by 28, the number of cases in each quartile, provides the item means of 2.96 for the highest quartile and 1.75 for the lowest. The difference of 1.21 between these quartile means is the item's discriminative value. The critical ratio is 5.81.

TABLE I. CALCULATION OF ITEM DISCRIMINATIVE VALUE ITEM 13: THE FUTURE LOOKS VERY BLACK
N = 28 in each quartile

Response	Weight	Highest Quartile		Lowest Quartile	
		f.	f.w.	f.	f.w.
Strongly Agree	5	3	15	0	0
Agree	4	4	16	0	0
Undecided	3	10	30	1	3
Disagree	2	11	22	19	38
Strongly Disagree	1	0	0	8	8
Total		28	83	28	49
Mean			2.96		1.75

Item Discriminative Value: 2.96 − 1.75 = 1.21

An assumption underlying the use of the criterion of internal consistency, although not always clearly stated, is that items yielding statistically significant differences between extreme segments of the total score distribution are measures of a single variable. It is assumed that they are sufficiently homogeneous as measuring elements to be combined into a single score and treated as a unit. This raises the problem of whether statistically significant item discriminative values could be obtained if items measuring two uncorrelated variables were to be combined into a single score.

To provide an answer to this problem, two series of items were chosen which yielded total scores that intercorrelated about zero. These series were the 11 acceptable items in the scale to measure economic conservatism and the 11 acceptable items in the scale to measure family attitudes. The term "acceptable item" refers to an item on which the *strongly agree* response is assigned the lowest scor-

ing weight. The intercorrelation between scores on these two series was only .04 for a group of 100 young unemployed men.

These two series of items to measure virtually uncorrelated variables were combined into a single score and item discriminative values were computed on the basis of extreme quartiles as before. Table II shows the average discriminative value for items when quartiles were selected on the basis of score on the 11 family items, on the basis of score on the 11 economic conservatism items, and on the basis of total score on the 22 combined items.

TABLE II. AVERAGE ITEM DISCRIMINATIVE VALUES FOR 11 FAMILY ITEMS AND 11 ECONOMIC CONSERVATISM ITEMS WHEN QUARTILES ARE SEGREGATED ON BASIS OF SCORE ON EACH SERIES AND ON THE TWO SERIES COMBINED

(Controlled Sample Unemployed Men)
N=25 in each quartile

Type of Item	Quartiles Segregated by Score on:		
	11 Family Acceptable Items	11 Ec. Conservatism Acceptable Items	22 Combined Items
Family Acceptable	1.34	.08	1.03
Ec. Conservatism Acceptable	.08	1.21	.78
Combined Items			.91

Items in the family series yielded an average item discriminative value of 1.34, when quartiles were selected on the basis of score on the 11 family items alone. The average value was only .08 for these family items, when quartiles were selected on the basis of score on the economic conservatism series. The average value for the 11 family items was 1.03 when extreme quartiles were selected on the basis of combined score on the two series. The next line in the table shows the corresponding values for the economic conservatism items, when quartiles were selected on the three bases. The significant point is that the average discriminative value for the 22 combined items was .91, when quartiles were selected on combined score. The mean critical ratio is 3.36. Seventeen of the 22 items yielded critical ratios above 2.00. Eight of the family items and five of the economic conservatism items yielded critical ratios above 3.00. If one interprets the critical ratio in the usual way, the statistical probability that a critical ratio of 2.00 indicates a true difference is approximately 95 to 100. That so many items yield statistically significant discriminative values under these conditions indicates

that measurement of a single common variable cannot be safely inferred from the fact that items satisfy the criterion of internal consistency, as usually applied.

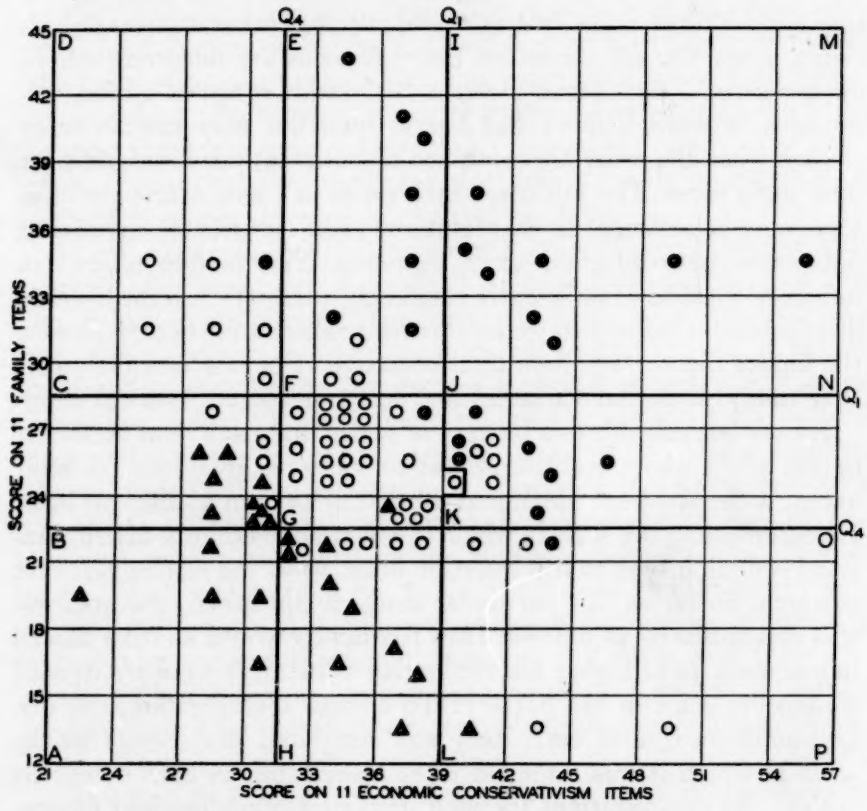
CORRELATION CHART

SCORE ON 11 ACCEPTABLE FAMILY ITEMS

VS.

SCORE ON 11 ACCEPTABLE ECONOMIC CONSERVATISM ITEMS.

$r = .04$



INDIVIDUALS SCORING IN:
Q_4 ON 11 F. ITEMS : AREA ABOP
Q_4 ON 11 E. C. ITEMS : AREA ADEH
Q_4 ON 22 ITEMS COMBINED : ▲

INDIVIDUALS SCORING IN:
Q_1 ON 11 F. ITEMS : AREA CDMN
Q_1 ON 11 E. C. ITEMS : AREA IMLP
Q_1 ON 22 ITEMS COMBINED : ●

How is it possible to obtain item discriminative values above the level required for statistical significance when two uncorrelated series are combined into a single score? The accompanying chart shows the correlation scattergram obtained when scores on these two

uncorrelated series were plotted. Scores on the family series are read from the Y axis, scores on the economic conservatism series from the X axis. The solid circles at the upper right show the individuals who scored in the highest quartile, when scores on the two series were combined. The triangles at the lower left show those who scored in the lowest quartile when scores on the two uncorrelated series were combined. The solid circles and triangles are far enough apart to yield many differences between quartiles on single items that are above the level required for statistical significance, even though scores on the two series are uncorrelated. In other words, if each series yields a fairly wide range of scores, differences between highest and lowest quartiles may remain large enough when the series are combined to yield statistically significant item differences. The influence each series of items exerts on total score may be sufficient so that its items yield statistically significant differences between total score segments. Yet, neither series can influence total score sufficiently to prevent a reduction in the average discriminative value of its items, when the combination occurs. Hence, the higher the average item discriminative value, the less likely it is that many dissimilar variables are being measured by total score.

It may be desirable in a battery of scales that each item be scored in the scale where its discriminative value is greatest. To what extent is the apparent ideational content of an item a sufficient basis for determining the scale in which it will yield maximum discrimination? A final answer to this question must await the results of future research. So far as this particular study is concerned, the question was reformulated as follows: How frequently would an item placed in one scale yield higher discriminative value, if it were transposed to another scale in the battery? To answer this question, the discriminative value of each item was computed not merely in the scale in which it was intended to be scored, but in each of the six scales. The computations for each item were made in eight groups, each containing 100 persons. This analysis yielded over 6,000 discriminative values for the 132 items in the battery of six scales.

Considering all groups and scales, it was found that the frequency with which an item discriminated better in another scale than that in which it was originally placed was surprisingly small. In only nine per cent of the instances were items found to yield higher discriminative values more frequently in some other scale. This sug-

gests that the apparent ideational content of an item is a reasonably safe guide to its placement within a battery of scales.

Certain items, however, which yielded their highest discriminative values in a given scale for one group were found to yield their highest discriminative values in a different scale for another group. Such an item is the statement, "No one cares much what happens to you." This item proved to be discriminating in the morale scale for all groups, but in three of the eight groups it was more discriminating in the family scale. It seems probable that each group contained a considerable number who agreed or disagreed with the statement on the basis of the degree to which they were adjusted in their family relationships. Those who care most about a given person's fate are in most instances the members of his immediate family. The item, then, appears to yield responses that reflect family adjustment, as well as morale in its more generalized aspects.

It appears probable that some items discriminating in more than a single scale evoke reactions which reflect different attitudinal configurations for different individuals. One item taken from O. Milton Hall's scale to measure occupational morale proved to be discriminating in both the morale and economic conservatism scales. This item reads: "There is little chance for advancement in business and industry unless a man has unfair pull." Those who feel that advancement in business and industry is not based primarily on merit or achievement may have their estimates colored by discouragement concerning their own personal prospects for advancement. This could account for the discrimination yielded by the item in the morale scale. Yet many who are not discouraged concerning personal prospects for advancement may still agree with the statement, if they feel that the prevailing economic system is unjust. Hence, either poor morale or economic radicalism may lead to agreement with the statement.

The items in our battery of scales were selected on the basis of their discriminative values for a group of 212 college students. A few items selected on the basis of experience in this group discriminated well only among college groups given the scales. There is one item of this sort in the economic conservatism scale. It reads: "Legislatures are too ready to pass laws curbing business freedom." This item yielded very low discriminative values when the scale was given to employed and unemployed groups containing persons whose

educational training did not go beyond the high-school level. It also proved to be undiscriminating among groups of high-school seniors given the scale.

Perhaps university students are more often aware of the historical conflict between liberals and conservatives as to the role government should play in relation to business enterprise. Opposition to legislative restrictions affecting business has been a traditional policy of economic conservatives for at least a century and a half. Economic liberals have demanded regulation of business to prevent continuance of unfair practices. University students who favor a *laissez-faire* policy may have interpreted this item to be an expression of their point of view with regard to legislative restrictions on business. The item differentiated sharply between college students whose total scores placed them at opposite extremes on the scale. Failure of the item to discriminate in other groups indicates that the discriminative merit of an item cannot be adequately determined on the basis of experience in a single group. When a scale is given to groups differing in education, the discriminative values for items cannot be assumed to remain constant.

Differences in the discriminative merit of items from group to group are manifest when items are ranked in order of discriminative value for groups of different composition. Items in the morale scale were ranked in order of magnitude of their discriminative values for eight different groups, each group containing 100 persons. The rank order coefficients were then computed between these rankings. The mean of the 28 rank-order coefficients obtained was .35. The low magnitude of these coefficients indicates that the rank order of items in terms of discriminative value varies widely from group to group. In consequence, some of the items which were eliminated on the basis of low discriminative values in the college group used to select items might have proved more discriminating in a wide variety of groups than some of the items retained. These findings warrant the conclusion that experience with items in a single group is definitely not a sufficient guide for item selection. To increase the probability that retained items will prove to be discriminating in a wide range of groups, two or more groups varying widely in composition should be given all of the items in the preliminary form of the scale. If items selected for retention discriminate in each group, the probability that they will prove discriminating in a wide variety of groups given the final scale is considerably enhanced.

DISCUSSION

AN ANALYSIS OF THE PROBLEM OF VALIDITY

RAYMOND V. BOWERS
University of Rochester

PROFESSOR SLETTO'S paper represents a contribution to an over-neglected task in sociology. It concerns the fundamental question of the applicability of specific methodological presuppositions to social phenomena. Only too frequently have social research techniques been used without the necessary concomitant investigation into their underlying logical foundations, and the applicability of these to the sense world. The field of measurement has suffered its share of this critical inertia in the past; consequently any attempt to examine the assumptions underlying a particular type of measurement should be encouraged by all serious research workers.

An adequate critical evaluation of such an analysis as Professor Sletto has given us seems to the present writer to necessitate two points of reference. First, the analysis should be evaluated with reference to the scientific tradition involved—in this case personality scale construction, and secondly with reference to the broader problem of the adequacy of this tradition in the light of scientific method.

Evaluated as an example of research in the field of personality scale construction, the study under review leaves no basis for serious argument. The author unearths a major assumption of the criterion of internal consistency, critically analyzes it with reference to concrete data, points out some of the pitfalls that lie ahead of the researcher who may use the criterion, and suggests a way to avoid these pitfalls. Any such experiment in technical analysis places its author among the more discriminating students of the scale tradition.

In concluding his paper, our author expresses the belief that "other methods for improving the technique to secure internally consistent measurement can be devised." The present reviewer entertains the same hope. Moreover, he is convinced that such technical analysis as is represented here is *necessary*, if progress in measurement is to be effected. At the same time, he has grave doubts that technical analysis is *sufficient* to produce this hoped-for progress. Although the evidence that originally led to these doubts came from other sources, the personality-scale tradition will be utilized here as it seems to

substantiate these doubts. But first let us review some fundamentals in science.

No matter how erudite our definitions of science may be, they reduce fundamentally to the simple proposition that science is an attempt to discover an assumed order underlying the perceptual chaos of the sense world. This intellectual task involves two mutually dependent divisions of scientific endeavor; first, the construction of a theory which makes one kind of order out of a given field of phenomena, and second, the construction of a research tradition to verify the elements of that theory.

Both of these tasks involve specific contents and specific tools to handle these contents. The contents of research are the phenomena of that part of the empirical world involved, and the tools of research are the research techniques. The contents of theory are the statements or generalizations about these phenomena, and the tools of theory are the various logics, both qualitative and quantitative. A mature scientific theory is a logically interrelated body of propositions about the sense world; and mature scientific research, a quantity of discrete experiments on sense world phenomena attempting to verify these propositions.

On the basis of this logic of science two questions can and must be asked about every scientific generalization. First, is it logically related to other generalizations in the science, or, in other words, is it a valid element in some scientific theory? Second, what is its empirical probability, or, in other words, has it been verified? Putting the matter simply, the validity of a scientific generalization—or scientific law, if you prefer the more classical term—depends upon whether it does or does not fit into some conceptual matrix. Its verifiability depends upon whether it does or does not fit its sense world matrix.

When we attempt to analyze research techniques, we find an analogous situation, in that two questions have traditionally been asked every research technique, particularly scales of measurement. First, does it do the job it was constructed to do—in other words is it valid? Second, does it do a consistent job—in other words is it reliable? Moreover, it is safe to say that these questions have caused considerable anxiety among serious research workers in sociology, although the problem of reliability has been solved much more satisfactorily than the problem of validity. Since Professor Sletto's paper deals with one form of the problem of validity the rest of this

discussion will be devoted to this problem. It is the thesis here that validation in social scales of measurement is unsatisfactory because its rationale has not been clearly understood, and the present writer would like to offer an analysis of the problem in the light of the logic of science just mentioned.

As ordinarily conceived, validity is the measure of the relationship between a scale and some outside criterion. But what do we mean when we say that a certain test is a valid measure of intelligence if it correlates highly with school grades and teachers' estimates of the pupil's abilities? To answer this question significantly we must elaborate our previous description of validity in the realm of scientific theory. Within the conceptual realm the only thing that can be said of any proposition is that it is or is not logically consistent with other propositions. If it is logically consistent with them, it is valid in that system of propositions or conceptual matrix. If it is not, it is invalid. If there is no organized system to which it may be referred, there is no basis for judging its validity, and it must be labelled non-valid. In other words, validity is logical consistency within a conceptual matrix, and has nothing to do with empirical truth. The empirical truth-probability of the proposition is determined by research verification.

Returning to the intelligence test example, we say the test is valid if its results correlate highly with school grades and teachers' estimates. Are we not saying that we have a common-sense intuitive conceptual matrix or theory of intelligence, professionally approved indices of which are school grades and teachers' estimates? If the intelligence test correlates highly with these indices it also becomes an index of this intuitive conceptual matrix and is considered thereby a valid measure of intelligence. In other words, the validity of this scale is its believed consistency with our ideas of intelligence. However, since the conceptual basis for the validity is intuitive, the validity of the scale cannot be other than intuitive, no matter how refined and quantitative the measures of this attribute may be. As Professor Waller has recently said, "No virtuosity of technique can compensate for want of understanding." In terms of a substantial scientific position this intuitive validity is non-validity.

Analysis of scales of measurement in the more advanced sciences will yield illustrations of my point. The thermometer is built to measure heat. It is a valid measure of this variable because its construction is based on one of the basic propositions of heat theory,

that substances expand or contract with changes in temperature according to known laws. If a thermometer was constructed on some principle conflicting with this proposition, it would be invalid. If there was no theory concerning heat, any attempt at building thermometers would perhaps be ingenious, but would have no explicit basis upon which its validity could be judged. Every research worker would have his own intuitive idea and the number of thermometers would probably approximate the number of personality scales in modern social science.

Proceeding to the personality scale tradition, the analysis of validation yields a picture much like that found for the intelligence tests. As examples we might discuss two methods of validating such scales, the correlation with other tests method and the rating method. When we say that a scale is valid if it correlates highly with some other scale purporting to measure the same variable, we are not avoiding the central problem of validity, which is what the scale really measures. We are merely saying that it measures the same variable according to a certain probability, but what that variable is remains intuitively known at best and will be interpreted the same only by those whose intuitions are comparable. Obviously this method of validation is levelling ignorance rather than eliminating it.

Validation by ratings has a similar weakness. When a researcher chooses a group of statements for some scale, such as one for radicalism, his selection is based upon his insight or intuition into this social phenomenon. On the basis of his experience he believes that these statements represent radical and conservative attitudes, or, in other words, constitute a representative sample of the universe of expressions of radical and conservative attitudes. When he submits these statements to judges, he is merely asking them to give their intuitive judgments as to the ideational content of these statements, so that in the end he has a scale validated on the basis of the average intuitive skill of himself and his judges. Such intuitive validity is not susceptible to any except common-sense tests and will, of necessity, vary from person to person, from group to group, and from culture to culture. Again we are averaging unknowns, and unknowns which are quite likely to mirror the unconscious cultural biases of the judges. If now we apply statistical devices to this procedure, we cannot possibly come out with any greater validity. We can merely dress up the validity in professionally accepted

symbols. The scale itself remains non-valid by our present standards because its conceptual matrix is unorganized and ignored.

An example of this may be given. In Professor Sletto's paper it will be recalled that the validating criterion of internal consistency was used to check the author's intuitions concerning item-placement, and that it verified his decisions 91% of the time. He draws the conclusion that ideational content is a reasonably good guide for placement in a battery of scales. Another way of stating this conclusion is that the criterion of internal consistency does not appreciably alter the dictum of his intuition. It tells us that each item does a certain job with a certain probability, and if all probabilities over a certain figure are considered significant, then some of the items do that job significantly. However, it does not enlighten us one bit as to what that job is. The validity of the scale must still be determined. Pursuing the matter further, if still other statistical indices of validity were sought by correlating the results of the scale with the results of other scales, we still would not find our way out of this difficulty. We would merely be saying that, whereas the criterion of internal consistency tells us that each item in the scale does more or less the same job, the correlation with some other scale tells us that the two scales do more or less the same job. The job itself would still be elusively unknown.

A solution to this impasse is possible on the basis of the logic of science previously mentioned. The validity of a scale must wait for an organized, explicit social theory, so that it can precede rather than succeed the construction of the scale. Validity is mainly a question of prior intellectual labor, rather than *ex post facto* technical manipulation. Strangely enough, the scales that already exist in psychology and sociology have really conformed to this procedure, although it is not generally realized. When a scale constructor begins his task he has some ideas about attitudes and some about the type of attitude he wishes to measure. We say he has some "feel" for his data. On the basis of this "feel" he selects a sample of statements which appear to him to represent that type of attitude. What we are really saying is that the researcher has a theory of attitudes, no matter how unorganized or intuitive it may be, and some intuitive hypotheses concerning the type of attitude to be measured. The statements that he selects are chosen on the basis of this theory and these immediate hypotheses. This validation is determined at the beginning of the work on the scale and effected during its con-

struction, just as in the case of the thermometer the validating strategy is determined before the scale is built. In other words, scales of measurement are rational tools dependent for their validity upon scientific theory. So long as this rational foundation is intuitive the scales are not likely to be effective instruments. What they are measuring will not be clearly known, at least in any communicable way, and the scientific opinion will differ from intuition to intuition. When the underlying theory is explicitly formulated it will be possible to have explicit validity. Then personality scales and measurement generally in sociology will have resolved the present impasse.

Hence it is the conclusion of the present writer that although such technical analysis as Professor Sletto has given us is *necessary* for progress in scale construction, it is not *sufficient* to effect that progress. Progress will come only when this is accompanied by logical organization in the field of scientific theory. When theory becomes an explicitly formulated body of propositions, and when scales of measurement formally utilize one or more of these propositions as their rationale, then true validity will have been achieved. However, the responsibility for the present impasse cannot legitimately be laid at the door of the research worker. The responsibility lies rather with the social theorist, who has traditionally preferred to remain a scholar of historical social theory rather than become an organizer of the contemporary empirical theory that is emerging from research. That technical virtuosity which the theorist frequently belabors is really the researcher's desperate attempt to compensate for the lack of an adequate theoretical foundation for present-day social research.

ASSUMPTIONS AND METHODS IN ATTITUDE MEASUREMENTS

CLIFFORD KIRKPATRICK
University of Minnesota

THERE HAS been increasing interest in the problems of attitude measurement. Bain, Droba, Roback, Sherman, Watson and others have prepared useful summaries of recent developments.[1] It is to be hoped that sociologists will continue to investigate this fertile field with open minds, with due precautions against stereotyped procedures and with a sense of perspective.

There is reason to think that a genuine antithesis exists between certitude and meaningfulness. The transmissibility and verifiability of the quantitative-analytical approach can be purchased at too great a sacrifice of the meaning and insight associated with the intuitive-configurational approach. There does seem danger at times that the range of scientific implications may be sacrificed to refinements of statistical technique. Frequently the use of one formula as compared with another in sociological research would have less effect upon ultimate implications than positing a different conceptual framework or a new definition of units.

In the interest of provoking discussion, eight questions are here raised concerning the basic assumptions of attitude measurement. Answers based on the writer's own thinking and research are given only to illustrate one possible approach to the problem.

QUESTION I: WHAT IS AN ATTITUDE?

Obviously much time can be wasted in laborious measurement, if there is no clear conception as to the nature of the thing being measured. Since the body of literature concerned with the definition of attitudes is too vast to be cited here, there is perhaps justification

[1] R. Bain, "Theory and Measurement of Attitudes and Opinions," *Psychological Bulletin*, Vol. 27, 1930, pp. 357–379; D. D. Droba, "Methods Used for Measuring Public Opinion," *Amer. Jour. of Soc.*, Vol. 37, 1931, pp. 410–423; D. D. Droba, "Social Attitudes," *Amer. Jour. of Soc.*, Vol. 38, 1934, pp. 513–524; A. A. Roback, "Character and Personality," *American Yearbook 1933*, pp. 871–874; Mandel Sherman, "Theories and Measurement of Attitudes," *Child Development*, Vol. 3. No. 1, March, 1932, pp. 15–28; Goodwin Watson, "Character and Personality Tests," *Psychological Bulletin*, 30, July 1933, pp. 467–487.

for an arbitrary definition, providing it is rigorously applied in subsequent analysis and testing. The general term *attitudinal behavior* may be applied to behavior having the quality of favorableness or unfavorableness toward some situation. Such behavior involves (a) gross somatic behavior, (b) overt verbal behavior and (c) internal somato-psychic patterns of thought and emotion. An *attitude* may be defined as internal somato-psychic behavior, the essential feature of which is an emotional-ideo-verbal system favorable or unfavorable to some situation. An emotional-ideo-verbal system then, as one component of attitudinal behavior, implies, on the psychic side, certain feeling states which diffuse through a train of ideas and images and, on the somatic side complex, physiological disturbances which accompany subvocal verbal responses. From this point of view attitudes are identical with sentiments, prejudices, wishes and emotionalized stereotypes. The dualistic terminology is adopted merely to aid in precise description.

A social attitude may be defined as an emotional-ideo-verbal system favorable or unfavorable to some social situation. A dislike of spinach is an attitude but a hatred of Communists as a menace to the social order is more specifically a social attitude. The essence of the anti-Communist attitude is a pattern of images, ideas, reveries, and perhaps half muttered invective accompanying somato-psychic reactions usually designated as anger, fear and disgust.[2]

If such a point of view is adopted certain implications follow. (1) Knowledge of an attitude or any other somato-psychic process in another person is always indirect and based upon inferences from various types of data. An attitude, however defined, is essentially a hypothesis, and the scientist should be willing to guide his critics back to the primary data and along the paths of inference which he has pursued.

(2) Attributing the quality of favorableness or unfavorableness to the emotional-ideo-verbal systems of another should not preclude recognition in the observation technique of ambivalence in the sense of rapid alternation of favorableness and unfavorableness components in the emotional pattern. There should also be recognition of what might be called "no attitude," namely, an absence of either favorableness or unfavorableness. A subject should not be forced

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[2] C. Kirkpatrick, "Content of a Scale for Measuring Attitudes Toward Feminism." To be published in *Sociol. and Soc. Res.* See D. D. Droba, "The Nature of Attitude," *Jour. Soc. Psych.*, Vol. 4, 1933, pp. 444–463.

by suggestion or by the nature of the measuring instrument to record a conviction which he does not feel.

(3) If it be held that an attitude implies favorableness-unfavorableness toward a specific situation it follows that items on an attitude scale should express favorable or unfavorable emotional reactions rather than factual relationships.[3]

Question II: Are Attitudes Continua or Patterns?

It has frequently been assumed that attitudes toward complex social movements such as Communism, Birth Control or Feminism may be described by simple favorableness-unfavorableness continua. By definition favorableness or its opposite is an essential quality of attitudes as abstracted from a total configuration. It is questionable, however, whether an individual can be meaningfully placed on a unilateral continuum with reference to an unanalyzed and heterogeneous pattern of issues. It is true that an individual could be asked to express on a five-point scale his favorableness or unfavorableness toward life. But his standing on such a favorableness continuum with reference to such a general concept would tell very little. The most that could be learned would be his favorableness to that which he understood by the abstraction "life." A favorableness self-rating toward Feminism might be a reaction to a misunderstanding of Feminism as a historical movement. There is much to be said for preliminary conception of specific attitudes in relation to a pattern of related issues. These specific attitudes may have themselves a pattern which includes "no attitude" components and ambivalent components with respect to certain issues.

Question III: Are Attitudes Part of Culture?

It would seem that patterns of social attitudes are personality counterparts of culture patterns evolved in connection with historical movements. Feminism, for example, consists essentially of attitudes on various issues in the minds of a number of people. A Feminist on the other hand is one who has emotionalized-ideo-

[3] Elsewhere the writer has pointed out that items used in attitude scales have not all been worded in evaluational form. See C. Kirkpatrick and S. Stone, "Attitude Measurement and the Comparison of Generations," *Jour Appl. Psych.*, Oct., 1935. The statement, "Birth Control Increases the Happiness of Married Life" in the scale prepared by Wang and Thurstone might be checked by one who accepted the fact but who was unfavorable to birth control as a sinful means of attaining happiness. Dr. Wang has himself made penetrating suggestions in regard to the form of items. See "Suggested Criteria for Writing Attitude Statements," *Jour. Soc. Psych.*, Vol. 3, 1932, pp. 367–373. See also D. H. Kulp, "The Form of Statements in Attitude Tests," *Sociol. and Soc. Res.*, Vol. 18, 1933, pp. 18–25.

verbal reactions favorable to the values or culture traits which are components of the Feminist pattern. The attitudes that an individual possesses are best designated by the name that culturally and historically has been applied to such attitudes. It may be contended that in the case of many tests of social attitudes there is validity in proportion as the test reflects accurately the issues that constitute the cultural components of an emergent culture pattern. To select propositions at random for an attitude test, trusting statistical techniques to establish reliability and validity, may mean that the test becomes merely a test of what the experimenter thinks is Feminism, Communism or Pacifism. If the problem is evaded by focusing each proposition on the key word (Feminism, Communism or Pacifism) then the test may be a test of the subject's attitude toward what the subject thinks is Feminism, Communism or Pacifism.[4]

This general point of view assumes that it is important to determine in the first place just what is being measured and to strive for validity through the original selection of an item pattern, rather than to prepare a shot-gun charge of items and then try to find out what they measure by giving the scale to a variety of groups. Much validation in attitude research seems to consist of correlating unknown with unknown, and then demanding consistency, where inconsistency may exist by virtue of inconsistent traits that have been brought into the same complex by historical circumstances.

With these considerations in mind the writer attempted to prepare a "belief-pattern" scale that would accurately reflect Feminism as a culture pattern together with the opposing anti-Feminist or patriarchal pattern. From an extensive examination of the issues expressed in the literature of Feminism and from resolutions of Feminist organizations a semi-objective outline of forty Feminist issues was prepared, and divided into four categories each containing ten issues. Some 386 evaluational propositions, pro and con, were prepared on cards. These cards were classified as to categories and issues. From the statements whose classification met certain standards of agreement on the part of judges, three forms of the scale were prepared.[5] Each form consisted of eighty items including a

[4] It is true of course that in the Thurstone method faults of inclusion are corrected by the criteria of irrelevance and ambiguity but the same cannot be said for faults of omission. See Thurstone, "Attitudes Can Be Measured," *Amer. Jour. of Soc.*, Vol. XXXIII: 4, Jan. 1928, pp. 529–554.

[5] C. Kirkpatrick, "Construction of a Belief-Pattern Scale for Measuring Attitudes Toward Feminism," to appear in *Jour. Soc. Psych.*

Feminist and anti-Feminist proposition for each issue of the outline. Each item, therefore, was validated by agreement on the part of the classifying judges. Thus there was not only elimination of ambiguous and irrelevant items, somewhat as in the Thurstone method, but also fitting of items to a cultural pattern of reference. In contrast to an earlier study no formal attempt was made to equalize strength of working. It was assumed that a subject was Feministic in proportion to the number of Feministic items checked and to the infrequency with which anti-Feminist items were endorsed. Category and total scores were computed as the algebraic sums of Feminist (positive) items accepted and anti-Feminist (negative) items accepted. It was hoped by this method to measure an attitudinal pattern in terms of extensiveness rather than intensiveness. This raises another and most important question.

Question IV: What Is Measurement?

It is somewhat presumptuous to attempt a discussion of such a vast topic within the limits of this analysis. It has previously been suggested that a useful distinction can be drawn between quantitative variables expressing amount and what might be called qualitative variables expressing degree. This distinction, if granted, implies that there is an important difference between measurement and rating, or perhaps between counting-measurement and rating-measurement.

A *quantitative variable* may be defined as a number derived by a counting process, which expresses a multiple or sub-multiple of units which are conceptually equal and interchangeable without effect upon the relevant implications of the derived expression.[6] It may be argued that the term measurement, or at least the term counting-measurement, could usefully be restricted to the derivation of quantitative variables by the process of counting units which are equal and interchangeable with reference to the purpose at hand and which are added to yield multiples of such units. Measuring the population of a room to determine the number of chairs required is measurement in the above sense. The people in the room differ, but they are essentially interchangeable from the point of view of the seating problem. A body weight of 150 pounds is essentially a multiple expressing the number of pound weights that would balance a

[6] C. Kirkpatrick, "Statistical Studies of Personality and Personality Maladjustment," in *Statistics in Social Studies*, Stuart Rice, Editor, University of Pennsylvania Press, 1930, pp. 197–216.

particular human body. Pound weights exist to standardize and make interchangeable the conceptual pounds in the minds of different persons.

It should be noted that measurement, in the sense of the derivation of a quantitative variable, may be *direct*, or *indirect* by means of an index. Counting of people is measuring population directly by deriving multiples of units which are themselves parts of the thing measured. The person of 150 pounds weight may push a spring scale to a point marked 150 pounds. Here the movement of a needle on a dial is an index of weight and the measurement is indirect. A death rate, to cite one more example, may be an *index* of the health conditions in a particular locality. It should be recognized that all attitude measurement is indirect measurement in terms of an attitude index. There is no way in which attitudes as here defined can be directly counted by persons other than the introspective self-observer.

The unit counted whether used in direct or indirect measurement may be either *natural* or *artificial*. A person is a natural unit in measuring population. An industrial accident, on the other hand may be artificially defined as an injury necessitating the loss of at least a day's work. A ton-mile is more definitely artificial. A degree, a calorie or an erg are artificial units of temperature, heat and energy. Here effects produced constitute units in the indices of the thing to be measured.

It may also be noted that, in the process of measurement as here defined, scales may or may not be used. If used, a scale may be employed (1) as a conceptual instrument of classification, or (2) as a physical instrument of observation. In the former sense a scale is a continuum used in the classification of observations. Measurements of stature, for example, may be grouped in a frequency curve with reference to a scale or continuum of height. The use of a scale as an instrument of observation consists essentially in counting units in groups rather than individually. The use of a foot rule makes it unnecessary to turn an inch end over end to count out a multiple of the inch unit. Mile per hour units could be counted on a speedometer but the numbers on the dial group the units into convenient multiples with numerical labels.[7] The use of a scale, then, as an in-

[7] For a somewhat different point of view see: Chapin, F. S., "The Meaning of Measurement in Sociology," *Pub. Amer. Sociol. Soc.*, Vol. XXIV, No. 2, May, 1930, pp. 89–91. See also Kirkpatrick, C., and Stone, S., "The Measurement of Attitudes and the Comparison of Generations," *Jour. App. Psych.*, Oct., 1935.

strument of observation is simply a means of counting units by groups rather than singly. Another way of expressing the distinction between the use and the non-use of scales is to contrast counting by matching and counting in the sense of enumeration. This is the distinction which Professor Chapin apparently has in mind in contrasting measurement and enumeration.[8] As we have seen matching with a scale which is a quantitative variable is merely counting by multiples. It will be argued that matching with a scale which is not a quantitative variable is not measurement at all, or is at least rating-measurement rather than counting-measurement. To identify a micrometer scale and a social distance scale, and to contrast the weighing of sugar with gram weights as compared with gram weight multiples, seems to identify the dissimilar and to contrast processes which are essentially similar.

A *qualitative variable* may be defined as a numerical expression of a classification, in terms of degree, of qualities that are not expressed in interchangeable units. Rating or rating-measurement (if the term measurement is admitted in the sense of numerical description) may be regarded as numerical classification in terms of degree, rather than amount, so as to obtain qualitative variables expressing degree relationships. Person A is more beautiful than person B or more hostile to the New Deal. A certain operation X may be described as hurting less than operation Z but more than operation Y. An article of furniture may be rated as intermediate between a chair and a table. In all of these instances degree variation and degree relationships are implied by numbers or by adjectives.

Qualitative rather than quantitative variables are derived under two conditions. (1) Psychic conditions and processes are numerically described by qualitative variables, when multiples of external and interchangeable units are not derived as indices by an objective counting process. When the pain of a dental extraction is expressed as a qualitative variable in a rating of four on a scale of five, it is probable that no psychic units were counted by the subject. Interchangeability of such units, if they existed, would be questionable in the absence of observable external counterparts of the psychic units. A gram is a conceptual unit of measurement but the existence of external physical counterparts in the form of gram weights insures the comparability of grams as conceived by different persons. The number of screams of an obstetrical patient is a quantitative

[8] F. S. Chapin, *op. cit.*

variable which is a rough index of pain. Screams can be added to yield a multiple and are relatively interchangeable in view of their existence as stimuli to the senses of more than one observer.

(2) In the second place, qualitative variables come into existence when a comparison is made between unanalyzed configurations. Persons may be rated or classified as to degrees of physical maturity without analysis in terms of reference to quantitative variables such as age, height, weight, and the like. Classification and counting are not unconnected, for classification, in the sense of definition of units, is often a preliminary to counting. Nevertheless, expression by rating of degrees of more-or-less tends historically to precede expression of amount. In one sense the progress of science consists in translating qualitative variables into quantitative variables, although for purposes of condensation the process may be reversed.

It is contended, therefore, that a useful distinction may be drawn between quantitative and qualitative variables and between counting-measurement and rating. The criteria of distinction are (1) the counting process, (2) interchangeability of units, and (3) the presence in counting-measurement of an external objective unit, which is manipulated in the counting process to obtain a multiple rather than to express a numerical classification in terms of degree of more-or-lessness. It is fully granted that all units are perceptually or conceptually defined by human beings and have only relative interchangeability.[9]

There remain, however, the criterion of counting and the criterion of external units which standardize personal conceptual units and render them conceptually interchangeable. Such units tend to be interchangeable in proportion as they are themselves defined by counting processes and by antecedent quantitative variables. A gram, for example, may be defined in terms of the gravitational pull of a mass of water of certain dimensions at a certain temperature. The most important single criterion of distinction between measurement and rating is the ultimate referability of measurement back to a counting process which can be carried on by more than one person. Rating might conceivably, in certain cases, be more accurate than counting-measurement, in the sense of obtaining constant results and correlating with other variables, but significant differences in the two methods of numerical description would still exist. This general discussion raises a specific question.

[9] S. Rice, "Units and Their Definition in Social Science," *Social Forces*, Vol. IX, No. 4, June, 1931, pp. 475–478.

Question V: What Do the Units of an Attitude Scale Mean?

It has been previously suggested that the widely used and valuable Thurstone scales yield qualitative variables which are ultimately derived from rating rather than counting.[10] The judges' ratings of the Thurstone scale items as to favorableness constitute a numerical classification in terms of degree. Numbers are used instead of adjectives, but have little more meaning than if adjectives such as "favorable" or "very favorable" were used. The proposition, "I do not receive any benefit from attending church services, but I think it helps some people" has a numerical name (scale value) of 5.7.[11] The number 5.7 is not a multiple of any objective unit. It means little more than that the proposition is regarded in general as more or less hostile toward the Church than certain other propositions with different numerical names. A person who takes a Thurstone test accepts certain propositions and thus receives a numerical name for his attitude or degree of attitude. The numerical name is not a product of counting and could be replaced by adjectives.[12]

Wherever a rating process is used the question arises as to whether the interval units are conceptually interchangeable. There seems no reason to think that the interval between three and four constitutes a unit which is conceptually interchangeable with the interval between nine and ten, even for a particular judge. The interval of the so-called continuum between proposition A and proposition B is not necessarily equal-appearing to *all* of the judges. The equal-appearing interval as a psychic unit, if unit it may be called, is not ordinarily counted in the rating process. Most judges assisting in the preparation of an attitude scale by the Thurstone method would probably give an introspective report of comparison rather than of counting. Even if the intervals of a favorableness continuum were counted by a rater in a way analogous to estimating length by imagining a ruler turned end over end, the intervals in the minds of different persons could not be regarded as interchangeable in view of the lack of any physical counterpart of the psychic unit. A person might count throbs of pain and thus approach a quantitative variable, but inter-person interchangeability would be lacking.

[10] C. Kirkpatrick and S. Stone, *op. cit.*

[11] L. L. Thurstone, and E. J. Chave, *The Measurement of Attitude*, University of Chicago Press, 1929, p. 33; also p. 61.

[12] The same comment may be made with reference to the Likert method of test construction. See Rensis Likert, "A Technique for the Measurement of Attitudes," *Arch. of Psych.*, No. 140, 1932.

Does the writer's method of constructing "belief-pattern scales" have any greater validity than attitude-testing procedures based on the rating principle? It will be recalled that the scores of the Feminism test previously mentioned consisted of the algebraic sums of Feminist and anti-Feminist items. Certain contrasts with the Thurstone method may be noted which may or may not be advantages.

(1) Items were selected and classified with reference to a pattern rather than a continuum. This process tends to keep qualitative differences in mind and to facilitate analysis with reference to an issue pattern.

(2) There is some reason to think that the fitting of the test items to a cultural pattern gives the instrument a certain inherent validity.

(3) The scores on the "belief-pattern" scale were derived by counting proposition acceptances as units having objective existence. This procedure is more consistent with the theory of measurement here set forth than the Thurstone method. The scoring of a Thurstone scale involves a counting not of units but of values which are essentially numerical names rather than unit multiples derived by a counting process. A sigma item scale value is more directly referable to an ultimate counting process, but a very large and heterogeneous standard group is needed for validity.

(4) While the "belief-pattern" scores are quantitative variables derived by counting, it cannot be argued that they are necessarily made up of equal units. The 80 items expressed 40 issues pro and con. They resembled each other only in that they all bore positively or negatively upon Feminist issues according to the uniform opinions of judges. That they were on the average interchangeable with reference to the purpose at hand was suggested by the fact that three forms with differently worded propositions bearing on the same issues correlated about .90. It is true that scales constructed by rating methods yield similar reliabilities. It may be contended, however that a "belief-pattern" scale score has a more clear-cut meaning than a score based on rating. Let it be assumed that a score on a Thurstone scale is five. The question arises "Five what?" The answer is five intervals on an eleven-point scale which were supposedly equal-appearing to judges reared in a particular culture. The equal-appearing unit had no direct physical counterpart in the sense that a brass gram weight corresponds to the idea of a gram as a *unit* rather than as a symbol like the word "gram." A score of five

on the "belief-pattern" scale for measuring attitudes toward Feminism means that five Feminist propositions have been checked in excess of whatever anti-Feministic evaluational propositions may have been checked. The score has a common-sense meaning as a multiple of things having an objective existence and having in common the quality which is being investigated.

(5) Finally, a principle of extensiveness of response was substituted for the principle of intensiveness of response. An attempt was made to obtain the range of response to a more complete sampling of issues rather than to investigate intensity of response to a generality. It was assumed that intensity of conviction in regard to a configuration finds expression in lowered thresholds and an irradiation of receptivity. The acceptance of each proposition counted as one, in spite of possible differences in intensity of response to various specific issues. This procedure, which was consistent with the theory of measurement here expressed, seemed to yield average interchangeability of units. When sigma weights were assigned to the items a correlation of .94 was obtained with the simple test scores.

There is even reason to think that the use of rating scales may artificially suggest more the extreme reactions on the part of judges and subjects. When students were asked to double-check propositions about which they felt strongly, sixty per cent of the students double-checked less than three Feminist propositions out of forty.[13] It is not claimed that there is any general superiority in the "belief-pattern" method of scale construction, but at least questions are raised which seem worthy of further thought and investigation.

Question VI: Is Attitudinal Behavior Consistent?

This question seems basic to the whole problem of attitude measurement. There is reason to think that a certain amount of attitudinal inconsistency is to be expected and should be analyzed into various types. It is useful to distinguish between four types of inconsistency. (1) *Inconsistency of behavior forms* is inconsistency between three components of attitudinal behavior: (a) attitudes as here defined; (b) overt verbal behavior or opinion; and (c) gross bodily behavior with reference to the object or situation. Complete

[13] Counting double weight for double-checked propositions lowered rather than raised reliability coefficients. It is possible that Sletto's use of the Likert five-point scales, with uncertain meanings for the extreme scores, explains his inability to find consistent discrimination by his items between extreme quartile groups.

consistency between the three components is not to be expected. The attitude (as here defined), the word and the deed rarely constitute a perfectly integrated trinity. All these components are realities and each is an index of the other two in the sense that some inference or prediction can be made from one variable to another. Action, however, is not necessarily more important than attitude or opinion.

(2) *Verbal inconsistency* is another form of inconsistency frequently found. Opinions expressed or endorsed by a person at the same or different times may seem inconsistent to an outside observer because of choice of words while there is, nevertheless, consistency in terms of attitude, i.e., internal emotionalized meaning reactions.

(3) *Logical inconsistency* may be regarded as a form of inconsistency in which there is an expression of inconsistent meanings. The acceptance or expression of propositions which are logically inconsistent may be due to stupidity, carelessness and perhaps to suggestion. A person could not logically report himself as both male and female, report children older than himself or knowingly be both favorable and unfavorable at the same time toward a specific issue.

(4) *Inconsistency of attitudes* may be distinguished from logical or verbal inconsistency in that there is a genuine internal inconsistency of thought and feeling. This internal attitude inconsistency may be of three types: (a) In the first place, the internal emotionalized ideoverbal process or system may be subjected to temporal variation. (b) In the second place, there may be incomplete endorsement of the various issues, which by virtue of historical association are generally assumed to belong together as part of a social movement. (c) In the third place, there may be inconsistency of attitude when a person endorses issues which are component parts of culture patterns which historically have been in opposition to each other.

Question VII: Is There an Antithesis between Reliability and Validity?

Extensive use has been made of retest reliability and internal inconsistency as techniques of standardizing attitude scales. It is conceivable, however, that increased reliability may be at the expense of validity.

(1) Retest reliability would be increased by the presence of irrelevant propositions which evoke consistent answers as, for example, the proposition "One should eat enough to preserve health."

A test of a variable attitude which yielded constant retest scores would be not a good but a bad test.

(2) It is possible that there is danger in eliminating propositions because of their failure to discriminate between high total score and low total score groups. For example, to assume an extreme case, if Feminism were confused with femininity in preparing a collection of propositions, the high score and low score groups might differ chiefly with reference to feminity; and items bearing on Feminism might be eliminated because of their lower discriminating value until the scale lost its validity and approached a test of femininity.[14]

(3) When scales are artificially given an internal consistency through selection and rejection of items, there is danger that genuine inconsistencies of attitude on the part of subjects may be concealed. The writer attempted to avoid this danger by retaining all items that fitted the pattern of Feminist issues. Furthermore, a device known as an *inconsistency ratio* was worked out to measure the proportion of all items checked (in relation to a particular total score) which were balanced by a checking in the same category on the opposite side of the Feminist, anti-Feminist dichotomy. The inconsistency ratio may prove to be a valuable research tool for investigating "marginal cultural status."

It is perhaps an advantage of the belief-pattern method of scale construction that an opportunity is given for the endorsing of issues in contrasted cultural patterns and that a means for measuring such inconsistency is provided.[15] The method forces no inconsistencies since no propositions need be checked save those which the subject himself regards as expressing his pattern of attitudes. On the other hand, lack of any definite attitude is expressed both by non-checking and by contradictory checking of both propositions bearing on an issue.

(4) In the fourth place, there is the fairly obvious danger that standardization by item selection would bias subsequent studies of attitude integration. Rather low partial correlations were found between the sub-scores for the economic, domestic, political-legal, and

[14] Sletto has evidence, however, suggesting that this process would not go beyond a certain point. It was found, however, again and again by Sletto and Rundquist that items in certain scales, as for example a morale scale, discriminated better in other scales, as for example an attitude toward education scale. When items of various scales are shifted around, what does a particular scale measure and how many items from other scales can be added before its title should be changed? See R. Sletto, Ph.D. Thesis, University of Minnesota (in preparation).

[15] Kirkpatrick, C., "Inconsistency in Attitudinal Behavior with Special Reference to Attitudes Toward Feminism," to appear in the *Jour. App. Psych.*

conduct-and-status categories of the writer's scale. These correlations would doubtless have been raised artificially had criteria of interval consistency been applied, and perhaps genuine inconsistencies would have been concealed. To drop out items bearing on some issue not closely associated might have prevented the scale from properly testing culturally different groups of persons among whom this issue had some vitality and significance.

Question VIII: What Are the Criteria of Attitude Test Validity?

If there is cogency in the suggestion that reliability may be obtained at the expense of validity, the problem of attaining validity takes on new importance. According to the preceding analysis, attitude indices should correspond as closely as possible to the emotionalized ideo-verbal patterns themselves, even to the point of recording ambivalence, inconsistency and modification of attitudes.[16]

There is probably more value in comparison of scores of groups of fairly well-known attitudinal status than in methods based on interval consistency. Analysis of item discrimination with reference to validating groups seems appropriate and desirable in many cases. It is probable, where interest is focussed more on the so-called personality traits, such as morale, inferiority feeling and generalized radicalism-conservatism, that correlation of test scores with case history data is desirable.[17] Where the attitudes refer to fairly definite culture patterns, such as Communism and Feminism, the method here described of fitting the pattern of test items to a pattern of cultural issues seems to have advantages, although detailed discussion is not possible.

It is admitted that the eight questions here raised have not been adequately answered. Problems such as these here presented need to receive much open-minded thought, discussion and experimental investigation before attitude measurement can be established on a strictly scientific basis.

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[16] The writer found significant changes in score on the part of students following discussion of Feminist issues with a student of the opposite sex. See C. Kirkpatrick, "An Experimental Study of the Modification of Social Attitudes," to appear in the *American Journal of Sociology* for March, 1936.

[17] Professor Stouffer has done important pioneering work in this direction. See S. Stouffer, *An Experimental Comparison of Statistical and Case History Methods of Attitude Research*, University of Chicago, 1930.

A NEW CLASSIFICATION OF CULTURE AND A RESTATEMENT OF THE CULTURE LAG THEORY

JAMES W. WOODARD
Temple University

MOST OF what will here be said is implicit in current social theory, especially in the work of MacIver and Ogburn, but even in that of Coste, DeRoberty, and Bagehot, partially in that of Sumner and many others. We have the highest regard for these contributions and here attempt only a more precise statement.[1]

We would divide culture into three categories: the inductive (or empirico-logico-experimental) culture; the aesthetic (or appreciative-expressive) culture; and the control (or authoritarian) culture.

In the inductive culture, we would include all inductively arrived at bodies of knowledge and all empirically validated devices. It would, therefore, include such material objects as tools, devices, machines, and various utilitarian objects and instruments; but also such non-material items as crafts, skills, techniques, mathematics, logic, scientific method, and bodies of scientific knowledge. Thus this category cuts across the division into material and non-material culture and infringes somewhat even on the territory of the so-called adaptive culture. It represents one sub-division of what Alfred Weber and MacIver have termed civilization by contrast to pure culture.

The aesthetic or appreciative-expressive culture is almost identical with what Weber and MacIver have termed pure culture and with what Ogburn terms the non-material, non-adaptive culture. It consists of such material objects as paintings, sculptures, mystic ritual symbols and vestments, architectural forms, and the like; and such non-material items as dance forms, recreational forms, the folkways of purely social intercourse, and the purely aesthetic aspects of poetry, literature, music, ceremonial, and the like.

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[1] See also, James W. Woodard, "Critical Notes on the Culture Lag Concept," *Social Forces*, Vol. 12, March, 1934.

The control culture contains, as its name indicates, all those items which have a prescriptive force and a controlling or conforming influence on the members of the group. Thus it includes such non-material items as usages, conventions, mores, morals, supernatural sanctions, and laws and institutions (in the Sumnerian sense). It also includes the rationalizing ideologies and buttressing lores and myths built up around these. And it includes such material objects as become affiliated with these in a control function—such as patriotic emblems, the insignia and official vestments of the offices of authority, or coercive or conformative symbols or objects of any kind. Thus this division constitutes a second subdivision of the Weber-MacIver civilizational category; cuts across Ogburn's material and non-material classification; and bites deeply into his adaptive culture.

It must be evident that the same item may have different causative significance in different aspects or relationships. An ermine robe, for example, is in a sense of the inductive order of provability. Certainly it is demonstrably warm in cold weather. It is also a control device, however, when restricted, say, to royalty or worn as a badge by a privileged few in the race for conspicuous consumption. Finally, it is also an aesthetic object and represents a pure value, an end rather than a means, justifying its own existence.

These various aspects are in varying degrees true of almost any object or item in the culture. So that it is only in terms of processual and causal analyses that our classification becomes useful. In gaining this significance, our classification admittedly loses the objective ease of identification which the material and non-material categories afforded. Further, when we examine the sources and processes which produce the differences here distinguished, we shall find that most of them are involved, though in varying degrees, in all aspects of social and cultural living. So that the characteristics of these categories overlap and merge much as do, say, the distributions of stature of the three Caucasian sub-races. And we are in a sense reaching out toward the ideal-typical as a mode of abstraction. With this admission of their interdependence in mind, let us proceed to a further examination of the characteristics of these cultural sub-divisions.

We may contrast the three classes of culture in six regards: (a) the immediate source of beliefs and attitudes relative to them; (b) the nature of the entailed psychological processes; (c) the ultimate corrective acting upon the involved beliefs and attitudes; (d) the degree of provability or arbitrariness involved; (e) the rate of change; and

(f) flowing from differentials in this last, the direction of probable dependence—that is, which is the pace setter and which is the resisting element in the culture-lag pattern.

As to the sources of the involved beliefs and attitudes, there are three: (a) the nature of man, (b) the nature of the social order with its cultural heritage at the historic moment of its functioning, and (c) the nature of the natural-order universe (our knowledge of which only finally comes to include man as well as his social order). These sources are involved, in complex fashion, in all aspects of the culture; but each of them is the predominant source for one of our three divisions. Likewise, there are three types of mental processes involved; (a) inductive (or at least logico-experimental) processes involving inductive and empirical forms of proof; (b) authoritarian processes, involving "proof" by appeal to authority and a greater reliance upon pure deduction as such; and (c) *Selbstverständlichkeiten*, involving the self-evident aspect of things which, as aspects, are neither amenable to authority nor dependent upon inductive proof. Here too, the inductive method, over a period of time, gradually eats into authority and divests even the *Selbstverständlichkeiten* of that part which was so only because of illusion, delusion, projection, or reification.

The aesthetic culture. The immediate source of the beliefs and attitudes involved in the aesthetic or appreciative-expressive culture is the nature of man as a self-expressing agent and as a responsive agent to the aspects of his surrounding world. Both these aspects are evident in such activities as social intercourse, healthy bodily functioning, recreation, the dance, sex play, love-making, pure religion, art, literature, music, the joy sometimes found in craftsmanship, the sheer joy of discovery in scientific activities, the aesthetic element in ceremony and ritual, even the beauty which the Greeks found in geometry; and also to some extent in philosophy, remembering that there are both superficial and profound "aspects" of man's self, his world, and his relation to it. This takes into account that human nature itself may change somewhat through natural and artificial selection; and that personality and temperament are intimately bound up with the given culture, with status in the given social order, the degree of enlightenment or superstition contained in the cultural heritage, and with the influence of occupational conditions and institutionalized roles. Thus the social order and the inductive heritage condition this factor.

Also, art (and these other forms) becomes associated with other cultural and social activities and develops a series of secondary functions, usually of a control character as in the patriotic hymn, the aphorism, the problem play, and the like. Also inductive knowledge is reflected in it, as in naturalistic and realistic art, historical narratives, proverbs and the like which are a mixture of art and science, and sometimes also of mores.

But, while the ethos is so pervasive that even the most innocent art objects have *a* control aspect (e.g., the prim samplers and still-lifes of our grandmothers), it is no less true that, down under the differences in ethos, culture, social order, and language, such things as beauty, ugliness, love, hate, friendship, mystic values, and the great artistic, recreational, appetitive, self-expressive, and sociability motifs are very like. It is this "common-human" or "pure" aspect which we mean. We admit it is a difficult distinction. With many, e.g., the aesthetic acceptability of an ermine robe would be enhanced by the glamour cast round it by its control functions, just as the crucifix and the flag actually take on "unearned increments" of aesthetic value. In a recent book on reification the writer has given considerable stress to this factor.[2] But in the case of any of these objects it would not be impossible for one who is familiar with ethnocentrism, the roots of mystic meaning, and reification to separate out the sheerly aesthetic or expressive from the spuriously so. When he has done so, he will find himself in the realm of which we speak, the realm of *Selbstverständlichkeiten* independent alike of induction and authority.

For, since certain fundamental aspects of man's nature do not change, and since the *aspects* of both his knowledge systems and his social orders always contain certain general patterns of relationship to his own nature, there is always here contained a certain independence of cultural heritage and of social order. Although art (pure art, taken as an example) is at once the most individual, culturally and personally, of all man's creations, it is also the most universal. An harmonic triad or a Chopin sonata is what it is, aesthetically, regardless of whether the social order is communistic, socialistic, or capitalistic, the family institution polygamous or monogamous, and regardless of whether or not science has analyzed the mathematical relations of their overtones. Thus, while art, purely social relations, and expressive forms are in some measure interdependent with both

[2] James W. Woodard, *Intellectual Realism and Culture Change*, Hanover, Sociological Press, 1935.

the other sections of the culture, they also contain certain canons of their own which are subject neither to proof nor to authoritative command; which neither depend upon nor lay imperatives upon the social order or the inductive culture; which, in a word, just *are* to those who are responsive enough to see them.

The psychological processes involved in the appreciative-expressive culture are those of the order of *Selbstverständlichkeiten*. One responds or he doesn't; and neither logic, inductive proof, nor command can alter the situation. This part of the culture is transmissible only to those who are responsive. It does not have the universality of transmissibility or provability that the inductive culture has. Neither does this self-evidentness give rise to elaborate systems of internal logical consistency, as in the inductive culture; nor to a definitely provable direction of change. Aesthetic tastes, while in some aspects more universal than any other aspects of the culture, are subject to non-logical changes, accessions or declines, even to fads and fashions.

Thus, also, as to any ultimate corrective applicable to beliefs, values, and attitudes involved in this section of the culture, there is none, except as the inductive culture presently bites in far enough so as to undermine reification, ethnocentrism, delusion, illusion, and hallucinatory factors. This, however, presently assumes considerable importance.

For these reasons, this whole area of the culture does not enter into the area of provability; cannot have that accelerative rate of change which characterizes the inductive culture; nor a readily provable direction of change. At the most, one might posit a slow process of revealing further and expressing more adequately its basic canons. But even this would be doubtful. Certainly it is not cumulative in the sense of the inductive culture; at best it yields an arithmetic rather than a geometric curve.

Not this section of the culture itself, but the basic canons crystallized in it and the pure values striven for in producing it may carry implications that the social order is or is not functioning satisfactorily in so far as it does or does not, among other things, provide a maximum opportunity for such expressive, appreciative, and realizational activity and products. Hence this section of the culture (but more especially the processes which in their turn have produced it) enters into social problems and into the functional and evaluative aspects of science. It is less certainly a part of the culture-lag dilemma.

The control culture. Of the control culture we may say that the immediate source of the beliefs and attitudes involved in it is the given social order; that the characteristic psychological processes are those of belief in authority and of pure deduction with the status quo as the major premise; that the ultimate corrective for these beliefs and values is the two-fold one of the nature of man and his needs and the natural-order reality, including the historic situation of the group; that the inductive culture furnishes the most powerful effective corrective and the nearest possible approach to the ultimate corrective; that the forms and beliefs contained in the control culture are outside the realm of provability; that to the extent to which they are brought within it, they thereby become a part of the inductive culture; that the rate of change is very slow, the very nature of this section of the culture being to resist change, any basic change necessarily meaning replacement of one social form or order by another rather than having an acceleratingly cumulative aspect as in the case of the inductive culture; and, therefore, that, while the aesthetic culture erects standards in terms of which the control culture is judged, the chief dependence of the latter and its only provable and measurable forms of lag are those cases where changes called for by developments in the inductive culture are resisted by the control culture. Let us examine some of these matters in detail.

There is (1) a contained imperative in the nature of the group situation requiring (2) that there be a certain minimum of group-wide uniformity and predictability of behavior within the group in order (3) for the group to function as such in those aspects in which it must function as a whole (say in conservation, war, trade or diplomacy), in order (4) to have internal workability and order, and in order (5) that the individual within it shall be able to build up a set of consistent habits and attitudes without which the human being, lacking specific instinctive patterns, cannot survive. It is also inescapable, in terms of success in conflict with external groups (and in terms of retaining the economies just mentioned) that (6) the areas of conflicting individual and subgroup interests within any given group become defined, (7) that status relations be regularized in some way, and (8) that the unusually obstreperous individual urges and interests—principally property, sex and status (or ego)—be subordinated. These (9) must be subordinated (i) to this orderly workability, (ii) to the interests of the group as a whole, and historically also (iii) to the interests of the dominating class or sub-group.

(10) Since this prescribed forsaking of individual interests can never be validated to strictly individual reason, there grow up around it (11) rationalizing (rather than rational) validating ideologies, (12) the moral "ought," (13) supernatural sanctions, (14) approving and stigmatizing traditions, and (15) agencies of formal coercive conformity. The vitality of these is augmented by the fact, already alluded to, that (16) there is a certain immediate utility in *any* order and predictability. But there are also at work the conservative action of (17) habitistic inertia, (18) reification and ethnocentric naïveté, (19) the impiety and fear associated with violation of supposedly intrinsic supernatural taboos, (20) the extremely effective and sometimes ruthless opposition of the vested interests, themselves (21) situational emergents having a definite stake in the retention of any given status quo, (22) the integrative tendencies which produce mutual interdependences among the major institutions themselves, (23) consistency and interbuttressing of their rationalizing ideologies, and (24) mutual alliances (in effect) between the institutions and the other vested interests of the particular culture, even (25) accommodative mechanisms of rationalization, compensation, repression, displacement, sublimation, and the like that come to permeate the very personalities of the constituent members. Through all these factors, (26) the basic organizational mores (to combine a term of Prof. Waller's with one of my own) come to be woven into a firmly-knit integration of considerable internal consistency and workability, exceedingly resistant to change—in a word, a social order. By another term, the control culture.

To be sure, such an order has so many contained conflicts that it is never a completely stable equilibrium. But it always attempts to be. Indeed, since the forces which produced this section of the culture produced it as a means to order, its historical function has ever been to produce conformity. That is why we term it the control culture. Its very *raison d'être*, its essential nature, is to resist nonconformity, that is, to resist change. Since its prescriptions concern the will of the gods, the moral ought, and the settlement of clashing interests and desires, none of which is (at least as yet) susceptible of inductive proof or disproof, these prescriptions are (because of that default) always authoritarian in character, with the intolerance of heterodoxy, impiety and rational attempts at analysis which authoritarianism carries with it. And its buttressing ideologies are deductive and rationalizing rather than inductive and rational. It is

the realm of shibboleth, sentiment, stigmatization, idealization, preconception, scholasticism, and authoritarianism. Thus the psychological sources of attitudes with respect to this part of the culture and the intellectual validation of them, as well as its effects relative to social rigidity and social change, are the direct opposite of those of the inductive culture. Many factors are involved in this, as we have shown. Perhaps the ultimately diagnostic factor is that the one realm is subject to inductive proof or disproof; the other is not. Since there must be *an* order, the latter cannot (as yet) do otherwise than to rely on force, authority, or superstitiously emotionalized preconception.

The inductive culture. We may next examine the inductive culture —tools, devices, skills, techniques, and bodies of scientific knowledge; even, to a limited but increasing degree, the efficiency, as means, of social institutions themselves. Here the immediate source of the values, beliefs, and attitudes involved is observation and experiment on the natural-world reality, the realm of identifiable cause and effect. And the ultimate corrective is that reality. Further, each invention or discovery facilitates further gains, so that there is a steady accumulation of traits and a progressive movement in the direction of the efficiency of means and at least the reality-contactedness aspect of ends themselves. Not only does change facilitate further change; but, because there are logical relationships within the theoretical systems here involved, new discoveries or corrections involve contradictions and points of strain, and lead to further observation, or actually demand further changes in the dependent portions of the theoretical structure. And new devices enter into endless new combinations of provable utility. Thus change is not only accumulative but acceleratingly so. To this section of the culture, especially, applies that formula which correctly describes the contained accelerative-accumulative tendency of the culture process. This yields a geometric rate of change.

For in this section of the culture, further changes that are "necessitated" can be more readily met than in other sections, are more readily received, and more readily transmissible; and hence the whole process of change is here most in evidence. It is in this field that the group most readily and most confidently accepts the new, because it is in the realm of the provable or rationally probable. It does so also for the further reason that this is the realm of means by contrast with ends, and the efficacy of means is a demonstrable affair.

It is the realm of "ends" of *belief* or certitude, however; and this fact, coupled with the self-validation of its inductive method, makes it uniquely a pace-setter among the three sections of the culture as well as, in a special sense, the arbiter of *all* questions of cogency. This is especially so since, down through time, we come to gain inductive knowledge of, and a certain control over, the nature of man and the formation of personality themselves; and since we also gain understanding, predictability, and modes of inductive functional evaluation of social processes and institutions as well. It is for these reasons that this is the section of the culture of markedly accentuated cultural acceleration, steadily eating up the control culture and even in a measure modifying the aesthetic-expressive culture.

Thus the inductive culture proceeds with an inexorableness that neither authoritative command nor personal preference can stop. This is because of the nature of the psychological processes which are involved. These are here predominantly inductive and logical. Thus, though in a quite different sense from the aesthetic culture, this section of the culture carries its own proof—in this case, inductive and rational validation. Indeed, there finally emerges here a high disdain for any other method, a complete disregard of authoritarian pronouncements as such.

Further, the content of physics, chemistry, biology, and latterly, even psychology, sociology, economics, and political science, carries its own rational proof and is what it is, regardless of what the social order is. From both the aspects of accumulation and diffusion, this section of the culture is a rapidly changing and readily transmissible one, regardless of the participating individuals. Carrying its own rational validation, it is not tractable to the efforts to conform belief on the part of the control culture, which flows from the given status quo, but presently produces such material and psychological changes that the social order and controlling belief-systems themselves must undergo the indicated appropriate changes.

Relations between them. In the past, social-cultural change has been largely at the hands of one sub-division of the inductive culture, the economic forms and processes; so that schools of economic determinism have found ample historical evidence to support their contentions. But changes in economic processes, and even in economic institutions, are beginning to flow from science more and more; so that it is quite possible that the scientific part of the inductive culture will one day supersede the changes in economic forms as the pace-

setter of culture change and release the aesthetic culture to a fuller realization. But certainly the inductive culture as a whole will always have the pace-setting function. For since it flows from the natural-order reality as progressively approached by the inductive method, and since it is slowly extending to scientific knowledge of man and society themselves, it is at once the pace-setter for, and the nearest possible approach to a final corrective or criterion of the other parts of the culture.

In the inductive culture, one can speak with confidence of superior and inferior as applied to means and agencies and of true and false as applied to ends, attitudes, and beliefs. Also, there is a "must-ness" to the march of the inductive culture that is not there in the others. Thus we do not hesitate to identify this section of the culture as the major determinant of all social and cultural change. This statement is made within the qualifications imposed by the fact that the culture is a complex unity produced by an infinite number of factors interacting in the most devious ways. But although even a biological organism is an integrated unit, there is established in it a gradient of metabolic change and control (the physiological gradient of Child and others) in terms of the dominance of the area of greatest stimulation, responsiveness to outside influences, and rate of metabolic processes. Such an area, relative to the cultural integration (not, necessarily the political or economic, or *social* integration) is the inductive culture. We are not falling into the particularist fallacy when we emphasize its major role and its pace-setting action.[3]

While we have called the rate of growth of the inductive culture a geometric one, it is admittedly not as precise or as smooth as a true exponential curve. The actual curve of this section of the culture would seem to be a series of accelerating curves which decelerate again as they approach an upper limit of stability, this series itself seeming to fall within the curvature of a larger curve of the same type. With some tentativeness, we would say that the upper asymtote of this larger quasi-exponential curve is probably determined by the two-fold category of adjustment and reality-contactedness. The mediate asymptotes of the smaller curves which occur at historic points in this larger evolutionary process would seem to be the upper limit of applicability of the fundamental discoveries and inventions then in process of discovery, application, and exploitation.

But the functional appropriateness of the control-culture forms is

[3] Nor is this to assume that society is an "organism."

relative to the conditions under which the group lives; and the effective cogency of its rationalizing ideologies is relative to the inductively established bodies of knowledge accumulated in the inductive culture. Both these conditions are altered from time to time by the accelerating inductive culture. At times, changes in the inductive culture all but completely change the conditions to which the group's institutions and mores are supposed to be adapted, and the mental outlook in terms of which both the social order and its rationalizing ideologies will be judged. But when the corresponding changes begin to intrude, they disturb the uniformities which it is the very function of the control culture to maintain. And the historic answer of most groups to the problem of called-for change has been to buttress the status quo still further against the very changes called for, i.e., against the chaos and unpredictability occasioned by the inevitable transition period. Thus the *initial* reaction of the control culture, when change is called for in it, is to brace its feet the more vigorously against that very change. This arises out of its own essential nature.

For there would be no significance in one part of the culture changing rapidly and another very slowly except that the two are functionally related and interdependent. But the consistency achieved by the rationalizing ideologies of the control culture is constantly disturbed by the implications of the accumulating inductive knowledge. And changes in the economic *processes* after a while demand changes in the economic *institutions*. And, still further, correlative changes in the shifting class interests, the make-up of the group, its mobility, its realigned interests, needs, and numbers, its individuation, and its complexity, call for changes in the governmental and familial institutions. At the same time, all of these factors, plus science, call for changes in morals and religion. But all of these changes are resisted out of the complex network of attitudes, ideologies, sanctioning agencies, and vested interests involved in the support and enforcement of the control culture of the given status quo.

Thus there is a contained tendency in the nature of the psycho-socio-cultural process (a) for the inductive culture to change at an ever-increasing rate; (b) for these changes to call for changes in the control culture; (c) for the latter, because of its very nature, to resist these changes; and (d) for there to be, not merely a tendency, but a constantly aggravated tendency toward maladjustment and cultural lag.

This tendency toward more and more aggravated forms of culture lag as the rate of culture change (in the inductive culture) becomes more and more rapid is not entirely dismal, however, in its implications for the future. It can be radically minimized by the self-conscious abandonment of authoritarianism and traditionalism in favor of the inductive method wherever the latter is in the least applicable to the solution of a problem; by minimizing as far as possible the blocking action of vested interests (this, of course, is itself a tremendous task); by subjecting religious belief-systems to the test of scientific knowledge and analysis and handling religious taboos quite without gloves when they interfere with humane and rational solution of social problems; and by replacing moral preconceptions as rapidly as possible by scientific evaluations, i.e., by inductive appraisals of functional appropriateness.[4] For these three—vested interests, supernatural taboo, and authoritarianism as opposed to induction—are the diagnostic points of difference between the two sections of the culture; other factors, though varying in degree, are present in all three sections.

In part, however, this tendency toward culture lag is a self-contained one and results from the fact that the one part of the culture *is* susceptible of objective validation while the other (as yet) is not. Hence the control culture is clung to even in the face of recognized shortcomings far beyond its time of appropriateness until, often, only violent revolution brings it closer to conformity with the underlying forms and processes upon which its own forms are dependent. This aggravation, however, is subject to a limited amount of rational modification. That is, when eugenics and education (both of which are, of course, obstructed by these same forces) shall have given us a whole population capable of complex and difficult decisions, we may then be able to accept quite philosophically the view that there is bound to be a certain amount of chaos and confusion incident to the process of social-cultural change. We may then see that it is better, through freedom of speech, freedom of organization, academic freedom, and the retention of democratic forms, to have a certain amount of chaos, inefficiency, and pulling at odds constantly present, rather than, through coercive imposition of the control mores, to gain the immediate and superficial effects of "law and order" at the cost, presently, of an explosive form of change. This

[4] We have dealt with this matter elsewhere. See James W. Woodard, "Critical Notes on the Nature of Sociology," *Social Forces*, Vol. 11, No. 1, October, 1932.

in turn could be better accomplished if so much did not depend upon the absence of internal chaos and inefficiency, when the group is faced with external competition and conflict. Thus the achievement of a rational social order waits in some measure upon that of a rational (above all, a peaceful) international order, itself so largely dependent upon already having done away with the contained pressure of capitalism toward imperialism.

However, there is a further contained tendency in these cultural processes themselves which of itself tends to lessen with time that tendency toward lag and maladjustment which, so far as we have yet traced the process, we have found to tend toward ever-aggravated forms. That is this: the inductive culture tends to increase at what is roughly describable as a geometric ratio—hundreds of thousands of years in the paleolithic for slight changes; tens of thousands of years in the neolithic for considerable ones; thousands of years since the emergence of the great civilizations for a tremendous accumulation; and, since the firm establishment of disciplined empirical techniques and inductive science (the last century or two), inconceivably great accretions in a hair's breadth of time. This accelerating accumulation is in no inconsiderable part at the expense of the control culture. Down through time, the inductive culture steadily eats up the control culture—faster and faster. Every indication is that, projecting this curve into the future, it will continue to do so—faster and faster. The rain gods fall before empirical techniques and scientific agriculture; food taboos give way to dietetics; magic is replaced by cause and effect; science undermines belief in the gods; the social and psychological realm, once regarded as outside the realm of science, are brought into it; physiology, biology, and psychology begin to replace authoritarian morals with rational practices; economics, political science, and sociology begin to bring the social order itself within the circle of rational analysis, predictability, and control; increase in populational size, in transportation, printing, communication, and mobility, yield individuation and an ever greater reliance upon objective and impersonally stated analyses of the social order itself and validations of it. All of which involve the undermining or the actual replacement of control culture by induc tive culture. To the extent to which this occurs, of course, culture lag is obviated, one element in the conflict having been assimilated quite into the other.

Thus, while there is a transitional tendency for culture lag to be-

come more aggravated, we think there are significant factors tending to minimize it in the long run, and that consciously chosen policies could greatly aid these factors.

DISCUSSION[1]

HOWARD BECKER
Smith College

PROFESSOR WOODARD'S proposal to discard (or at least to relegate to a position of subordinate rank) the antiquated material-nonmaterial distinction is of real importance. There seems no good reason why sociological theory should be able to make much use of a classification of culture based on museum utility. If a culture trait can be put under glass, it's material, and everything else is nonmaterial. This is one of those famous classifications that are true but useless. The threefold division into inductive, appreciative-expressive, and control categories seems likely to prove much more useful while being equally true.

This granted, however, I must raise some problems which, either because of time limitations or my own inability to offer satisfying solutions, will not be answered here. (1) Is "inductive" a good term for what Professor Woodard has in mind? Does it not contribute to the already great confusion brought about by the utterly fallacious antithesis, inductive *versus* deductive? (2) Is there not a certain epistemological question-begging latent in the assumption that through the inductive culture, and that alone, we are brought into closer contact with "natural-world reality," whatever that may be? Do we not find the most recent work in modern natural science supporting the contention that we now know no more about the nature of "reality" than in the remotest ages of the past, that technics and science have merely instrumental validity? (3) Does the so-called inductive culture carry "its own *rational* validation?" What is to be the criterion of rationality? Is not the inductive culture something that is simply *there*, and over against which we are much in the position of Margaret Fuller when she announced that she accepted the universe? (4) Is there any way whatever of determining which part of a given culture is the "pace-setter" for another part? It seems to

[1] The first one-third of Professor Becker's paper has been omitted for the sake of brevity (HB).

me that here we have the old Marxian logomachy to which Weber and Pareto, to name no others, should long ago have given an eternal quietus. Is it not much safer and sounder to rest content with the simple assertion of functional interdependence? (5) Does rigid suppression of change by the control culture have violent revolution or like explosion as an inevitable aftermath? Any close examination of pre-revolutionary configurations will show that not a continuance of rigid suppression but rather a *partial* weakening of the formerly rigid structure is the immediate prelude to the outburst. (6) Is there any historical evidence to show that it is possible to replace authoritarian morals by so-called rational practices among any appreciable proportion of the populace for any long period? May we not say that it is possible to free all of the people some of the time, and some of the people all of the time, but that we cannot free all of the people all of the time? Is there ever any escape from a fundamental irrationality of conduct when ultimate values are taken into account? (7) What is the warrant, apart from purely personal preference, for the coupling of the terms "humane" and "rational?" Is there any necessary connection? (8) Why should individuation for all, even supposing it to be possible, be the goal of sociological effort? Who is to decide whether the supposed advantages outweigh the undoubted costs? (9) Is there any warrant for assuming that eugenics and education will ever give us "a whole population capable of complex and difficult decisions?" And what would a society be like, in terms of net human happiness, where everybody was *uniformly* of an I.Q. five times as high, let us say, as that prevailing at present? Does not just this kind of wished-for egalitarianism, as we find it in our own democracy, make ambitious and unhappy climbers of us all? Does the quest for secular salvation have anything at the end of the rainbow but the fabled pot of gold?

To conclude: Professor Woodard's paper, in spite of the undeniable instrumental validity of his distinctions, is predicated upon an ingenious variant of the old rationalistic philosophy of history. He must demonstrate the validity of that philosophy of history if his program of action is to mean anything beyond a private pious wish. In order to do this he must show that opposing philosophies are invalid, and he must conduct this demonstration within a logical rather than a psychological frame of reference. Merely to say that a man is rationalizing proves nothing against his theory; that must be accepted or rejected on other grounds. The fact that someone

has undergone experiences of a peculiar character which he has then rationalized may mean that as a consequence he is able to direct our attention to previously unnoticed and entirely valid realms of sociological theory. Further, what is to prevent Professor Woodard's hypothetical opponent from asking, "Why, my dear man, are you rationalizing too?"

Let me recommend as a sack of ballast for any would-be sociological Icarus the reading of Aldous Huxley's *Brave New World.* I make bold to think that he will conclude that the sociologist must continue to distinguish between his function as a sociologist and his various roles in everyday life as it is lived. This may, to be sure, accentuate a certain compartmentalization of conduct that is inherent in all social action, and in so far forth I am demanding that the sociologist try to become a more or less sane schizoid. In other words, I hold that all sociologists should be mildly crazy, but mark this, crazy *in a particular way.* If we persist in mingling scientific analysis with programs of action, the result will not be schizophrenia, to be sure, but something that is perhaps even worse. We all know that no one is more completely integrated, less subject to mental cleavage and doubt, than the sufferer from paranoia. Let our program of action as a sociological society be the promotion of bigger and better schizophreniacs!

THE SOCIAL ACTION PATTERN OF THE PROTESTANT RELIGIOUS LEADER

JEROME DAVIS
Yale University

ON JANUARY 10, 1935, we sent out through the Religion and Labor Foundation a social action questionnaire to 100,000 religious leaders of twenty-two major faiths and denominations with a letter signed by fourteen religious leaders of the Catholic, Jewish and Protestant faiths. It should be understood that the purpose of this questionnaire was educational. It was designed to stimulate the thinking of the ministers on radical types of action, rather than to obtain scientific data. Consequently the responses left much to be desired when it comes to showing the total action pattern of the minister. Nevertheless the returns do have considerable sociological value.

There were 47 specific questions grouped under eleven general action patterns:

(1) Social legislation; (2) Civil liberties; (3) Coöperation with labor leaders; (4) Industrial disputes; (5) Right of labor to organize; (6) Aid to underprivileged groups; (7) Political activity; (8) Rural coöperation; (9) Jail service; (10) Membership in national organizations for social justice; (11) Willingness to organize the group or participate in social action; (12) Concrete examples of individual and group action.

Forty-seven hundred religious leaders replied. The largest percentage of returns in order were: Unitarian, Evangelical Synod of North America, Congregational-Christian, Friends, Presbyterian U.S.A., Northern Baptist, Jewish, Methodist, Episcopalian, Evangelical Church, United Brethren, Disciples, Lutheran, Methodist Episcopal South, Presbyterian U. S., Catholic, Reformed in America, United Presbyterian, Methodist Protestant, Reformed in U.S., Methodist Colored, and Southern Baptist.

The distribution of replies came from all over the nation. The South showed the least response; only 50 out of 22,000 Southern Baptists replied. Forty-five per cent of the returns were from communities under 5,000 and approximately one-fifth from municipalities over 100,000.

Roughly ninety-seven per cent of the replies came from Protestestant leaders. It is my personal belief that a much higher proportion of liberal rather than conservative leaders replied. In other words, it seems probable that the majority of Protestant clergymen are taking less social action than is represented by these replies.

TABLE I. PROGRESSIVE LEGISLATION

Will you, through the press, and the organization of public meetings, support progressive legislation, such as:

Types of Response	Old-Age Pensions?		Unemployment Insurance?		Child-Labor Amendment?		Public Ownership of Public Utilities and Basic Industries?		Will you support the items you have checked "yes" by public statement in the pulpit?	
	No.	%	No.	%	No.	%	No.	%	No.	%
No answer	442	9.4	486	10.3	481	10.2	614	13.1	602	12.8
Yes	3161	67.3	3126	66.6	3144	66.9	2172	46.2	2728	58.0
I have done this	141	3.0	123	2.6	145	3.1	97	2.1	207	44.4
Have, and will again	392	8.3	334	7.1	421	9.0	231	4.9	449	9.6
No	226	4.8	248	5.3	247	5.3	617	13.1	331	7.0
In doubt	203	4.3	261	5.5	147	3.1	793	16.9	172	3.7
Local situation prevents	34	.7	33	.7	26	.6	59	1.3	62	1.3
Does not apply	1	.1	—	—	—	—	14	.3	13	.3
Answers with comment	100	2.1	89	1.9	89	1.9	104	2.2	136	2.9

Table I shows the response of the ministers to old-age pensions, unemployment insurance, the child-labor amendment, and public ownership of public utilities and basic industries. It can be seen at once that the ministers are friendly to the child labor amendment;[1] Seventy-nine per cent, or 3,710 pledged themselves to work through the press and the organization of public meetings for this amendment. Roughly 1,200 less, 53.2 per cent favored public ownership of public utilities and basic industries. However, only 12 per cent had ever worked for the child-labor amendment in the past and only 7 per cent had ever done anything for public ownership of public utilities.

Table II shows the extent to which the ministers will support civil liberties. Approximately one-third of the ministers are willing to make available a place of meeting for those denied or restricted in their freedom of speech. Six hundred and twenty-four ministers say flatly they will not provide such a place, while 728 more are in doubt. Only 4.4 per cent have ever actually furnished such a place in the past.

[1] In reading these tables it is necessary to add to the "yes" response those in the "I have done this" and "Have and will again" to obtain the total who are willing to take such action.

Fifty per cent of the ministers are willing to get acquainted with the officials of the labor unions in their community. Thirty-seven per cent are willing to entertain them in their homes, but only 25

TABLE II. CIVIL LIBERTIES

Will you support the rights and responsibilities of free speech, assembly, and press, by:

Types of Response	Public statement and preachment, forum and writing?		Making available church, parish house, or synagogue as a meeting place?		Supporting financially or otherwise the re-trial of those who have been discriminated against in the courts?	
	No.	%	No.	%	No.	%
No answer	444	9.4	626	13.3	807	17.1
Yes	3206	68.2	1578	33.6	2323	49.4
I have done this	171	3.6	72	1.5	75	1.6
Have, and will again	469	10.0	135	2.9	156	3.3
No	117	2.5	624	13.3	317	6.7
In doubt	132	2.8	728	15.5	586	12.5
Local situation prevents	32	.7	332	7.1	116	2.5
Does not apply	15	.3	91	1.9	25	.5
Answers with comment	114	2.4	514	10.9	295	6.3

per cent are willing to invite them to speak to their congregation; and of the total number who replied only 4 per cent had ever entertained trade union leaders in their homes or elsewhere.

TABLE III. LABOR LEADERS AS GUESTS AND SPEAKERS

Will you:

Types of Response	Get acquainted with the officials of the labor unions in your community?		Entertain trade union leaders in your home or elsewhere?		Invite labor union leaders to speak to your congregation?		At a regular service?		At a special meeting?	
	No.	%	No.	%	No.	%	No.	%	No.	%
No answer	868	18.5	1176	25.0	1442	30.7	2043	43.5	1661	35.3
Yes	1889	40.2	1539	32.7	1000	21.3	504	10.7	1474	31.4
I have done this	290	6.2	103	2.2	96	2.0	65	1.4	84	1.8
Have, and will again	196	4.2	92	2.0	80	1.7	43	.9	75	1.6
No	265	5.6	501	10.7	843	17.9	1148	24.4	557	11.9
In doubt	233	5.0	481	10.2	469	10.0	317	6.7	345	7.3
Local situation prevents	392	8.3	352	7.5	301	6.4	204	4.3	172	3.6
Does not apply	485	10.3	260	5.5	258	5.5	213	3.5	200	4.3
Answers with comment	82	1.7	196	4.2	211	4.5	163	3.6	132	2.8

Most of the ministers are willing to try to find out the facts in cases of industrial disputes. Forty-seven per cent are willing to support the right of labor to picket and strike. Roughly 12 per cent are

TABLE IV. RELIGIOUS LEADERS IN INDUSTRIAL DISPUTES

If the facts lead you to believe that their cause is just, will you:

Types of Response	In case of an industrial dispute, will you: make an effort to get the facts and to acquaint your people with them?		Support the legal and ethical right of labor to strike, picket, and engage in mass demonstrations?		Secure or assist in securing pickets for the union?		Join in the picket line yourself?		Visit their strike meetings when workers are out on strike?		Provide strikers with food, clothing, fuel, and other basic necessities required to sustain the strike?		Offer your church or synagogue for strike meetings?	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
No answer	709	15.1	711	15.1	978	20.8	947	20.1	866	18.4	881	18.8	938	20.0
Yes	3096	65.9	2033	43.1	513	10.9	364	7.7	1943	41.4	1598	34.0	540	11.5
I have done this	83	1.8	69	1.5	17	.4	19	.4	61	1.3	68	1.4	19	.4
Have, and will again	192	4.1	137	2.9	23	.5	22	.5	92	2.0	86	1.8	14	.3
No	143	3.0	759	16.1	1855	39.5	2049	43.6	731	15.5	669	14.2	1661	35.3
In doubt	130	2.8	538	11.4	870	18.5	855	18.2	336	13.5	810	17.2	783	16.7
Local situation prevents	87	1.8	78	1.7	127	2.7	118	2.5	98	2.1	183	3.9	315	6.7
Does not apply	145	3.1	108	2.3	111	2.4	113	2.4	107	2.3	109	2.3	126	2.7
Answers with comment	115	2.4	277	5.9	206	4.3	213	4.5	169	3.6	296	6.3	304	6.5

TABLE V. THE RIGHT OF WORKERS TO ORGANIZE

Will you uphold the right of workers to organize in the following ways:

Types of Response	Circulating among the members of your congregation literature favorable to the cause of organized labor?		Advocating aggressively with workers their right and need for organization, and aiding union organizers by personal visitation and by public speaking?		Contracting with and recommending that church and synagogue membership deal with trade unionists or industries that employ union men?		Appearing before regional or city labor boards to aid in securing the full rights of labor in the local situation?		Giving publicity to the attempts of employers to destroy genuine workers' unions and to establish company unions?	
	No.	%	No.	%	No.	%	No.	%	No.	%
No answer	784	16.7	1019	21.7	1096	23.3	1018	26.7	1106	23.5
Yes	1683	35.8	1235	26.3	1067	22.7	2192	46.6	1711	36.4
I have done this	38	.8	60	1.2	51	1.1	30	.6	42	.9
Have, and will again	73	1.6	97	2.1	50	1.1	49	1.0	90	1.9
No	928	19.6	1035	22.0	1077	22.9	448	9.5	653	13.9
In doubt	635	13.5	859	18.3	913	19.4	540	11.5	764	16.3
Local situation prevents	161	3.4	163	3.5	168	3.6	187	4.0	122	2.6
Does not apply	117	2.5	71	1.5	75	1.6	74	1.6	71	1.5
Answers with comment	281	6.0	161	3.4	203	4.3	162	3.4	141	3.0

willing to assist in securing pickets for a trade union, and about the same number are willing to offer the church for strike meetings.

Thirty-eight per cent of the ministers were willing to circulate among the members of their congregation literature favorable to the cause of organized labor. Thirty per cent are willing to urge workers to form a union organization of their own, and 25 per cent are willing to recommend that the church deal with trade unions in their own contracts.

TABLE VI. FAIR PLAY FOR UNDERPRIVILEGED GROUPS

If there are considerable numbers of Negroes or other underprivileged groups in your community, will you:

Types of Response	Inquire into their chance for employment or relief and try to get fair play for them?		Encourage their leaders of character and intelligence by bringing them in contact with liberal white leaders?		Help their children get a fair share of the best educational opportunities?		Work against lynching by preachment and writing to your congressmen in favor of a federal law?	
	No.	%	No.	%	No.	%	No.	%
No answer	728	15.5	1017	21.6	954	20.3	837	17.8
Yes	2693	57.3	2546	54.2	2762	58.8	2984	63.5
I have done this	173	3.7	188	4.0	159	3.4	156	3.3
Have, and will again	256	5.4	268	5.7	243	5.2	277	5.9
No	51	1.1	84	1.8	53	1.1	69	1.5
In doubt	46	1.0	70	1.5	38	.8	79	1.7
Local situation prevents	201	4.3	160	3.4	132	2.8	69	1.5
Does not apply	423	9.0	269	5.7	255	5.4	147	3.1
Answers with comment	129	2.7	98	2.1	104	2.2	82	1.7

Table VI gives the response in regard to fair play for underprivileged groups. Apparently, when it comes to helping people along humanitarian lines that do not cause conflict, the ministers are much more ready to act. Two-thirds were willing to make inquiries to see whether Negroes or others had a fair chance at work or relief. Nearly three-fourths of all the ministers were willing to preach against lynching and write to their congressmen in favor of a federal law. This was true also of the Protestant ministers in the South.

Political alignment causes difficulty for the minister, since he does not like to be labeled partisan. About one-half would not agree to support any one political party. Some of the replies indicated that the writers wished to be independent and vote for the best man. On the other hand 12 per cent were willing to work actively for a

Farmer-Labor or Progressive party, 12 per cent for the Socialist party, 9 per cent for the Democratic party, and 12 per cent for the Republican party.

TABLE VII. POLITICAL ALLEGIANCE

Will you support actively in your community:

Types of Response	The Democratic Party?		The Farmer-Labor Movement, or a Progressive Party?		The Republican Party?		The Socialist Party?		The Communist Party?		A City-manager Plan?		The Coöperative Movement?	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
No answer	1962	41.7	1991	42.4	1861	39.6	1892	40.3	2203	46.9	1980	42.1	1626	34.6
Yes	338	7.2	489	10.4	467	9.9	381	8.1	36	.8	1174	25.0	1586	33.7
I have done this	52	1.1	36	.8	59	1.3	84	1.8	3	.1	62	1.3	48	1.0
Have, and will again	34	.7	26	.5	24	.5	112	2.4	7	.1	49	1.0	87	1.9
No	1370	29.1	1112	23.7	1254	26.7	1183	25.2	1586	33.7	637	13.6	450	8.6
In doubt	314	6.7	378	8.0	306	6.5	364	7.7	172	3.7	398	8.5	424	9.0
Local situation prevents	56	1.2	40	.9	47	1.0	44	.9	37	.8	93	2.0	36	.8
Does not apply	6	.1	4	.1	2	.0	1	.0	1	.0	14	.3	1	.0
Answers with comment	568	12.1	624	13.3	680	14.5	639	13.6	655	13.9	293	6.2	442	9.4

TABLE VIII. RURAL COÖPERATION

If you live in a rural community, will you:

Types of Response	Help the farmers to organize coöperatives?		Support a coöperative if one were organized in your community?		Join in preventing a farm foreclosure?	
	No.	%	No.	%	No.	%
No answer	1700	36.2	1564	33.3	1699	36.1
Yes	1446	30.8	1954	41.6	1301	27.7
I have done this	107	2.3	106	2.3	39	.8
Have, and will again	79	1.7	88	1.9	32	.7
No	223	4.7	134	2.8	325	6.9
In doubt	201	4.3	164	3.5	562	12.0
Local situation prevents	263	5.6	136	2.9	135	2.9
Does not apply	493	10.5	415	8.8	340	7.2
Answers with comment	188	4.0	139	2.9	267	5.7

In Table VIII the lack of response is partly due to the fact that in many cases the minister is working in the urban field. However, 35 per cent are willing to help the farmers organize cooperatives and 46 per cent are willing to support such experiments if organized.

In Table IX one sees the response of the clergy to assuming responsibility for a religious service, a good library and personal work

among the inmates of the county jail. Roughly one-fourth of all the ministers gave no answer to the question of the library and personal work. Only 8 per cent had ever helped in a religious service in the

TABLE IX. JAIL SERVICE

Will you see to it that the local jail in your county has:

Types of Response	A religious service each week?		A good library?		Someone to do personal work among the inmates?	
	No.	%	No.	%	No.	%
No answer	821	17.5	1200	25.5	1194	25.4
Yes	981	20.9	108	23.0	1028	21.9
I have done this	275	5.9	135	2.9	238	5.1
Have, and will again	103	2.2	48	1.0	75	1.6
No	133	2.8	173	3.7	142	3.0
In doubt	255	5.4	320	6.8	316	6.7
Local situation prevents	1076	22.9	927	19.7	885	18.8
Does not apply	442	9.4	361	7.7	355	7.6
Answers with comment	614	13.1	453	9.7	467	9.9

jail. Only 4 per cent had ever helped with a library. One-third of the ministers believe that their local situation prevents them from answering, although every community in the United States has some place to which prisoners are sent who have been arrested.

TABLE X. MEMBERSHIP IN NATIONAL MOVEMENTS FOR SOCIAL JUSTICE

Types of Response	Will you join one or more national organizations trying to promote social justice?		Will you give a definite percentage of your income to the support of such organizations?	
	No.	%	No.	%
No answer	926	19.7	1355	28.8
Yes	1883	40.0	845	18.0
I have done this	358	7.6	190	4.0
Have, and will again	123	2.6	85	1.8
No	432	9.2	916	19.5
In doubt	697	14.8	860	18.3
Local situation prevents	69	1.5	117	2.5
Does not apply	1	.1	1	.1
Answers with comment	211	4.5	331	7.0

Table X reveals that only about one-tenth had ever identified themselves with any organization which stood for social justice, outside their own denominations. About ten per cent declared that they would not join any such organization. Apparently they were

satisfied with their own denominational connections. On the other hand 50 per cent were willing to join such movements.

Table XI shows that roughly 45 per cent pledged themselves to organize their church or some other community group for the purpose of taking action in situations where social justice is involved, and 54 per cent agreed to continue in some of the above lines of action, even if it meant jeopardizing their position. It must be remembered that few ministers would be willing to say outright that they would not persist in an action which they believed right, if it meant jeopardizing their position. In fact it is rather surprising that 8 per cent were willing to admit they would not.[2]

TABLE XI. WILLINGNESS TO ORGANIZE THE GROUP OR PARTICIPATE IN SOCIAL ACTION

Types of Response	Will you organize your church or some other community group for the purpose of taking action in the situations where social justice is involved?		Will you, if the situation demands, persist in participation in some of the above lines of action, even if it means jeopardizing your position?	
	No.	%	No.	%
No answer	1058	22.5	995	21.2
Yes	1864	39.7	2195	46.6
I have done this	130	2.8	135	2.9
Have, and will again	134	2.8	178	3.8
No	474	10.1	394	8.4
In doubt	683	14.5	546	11.6
Local situation prevents	197	4.2	49	1.0
Does not apply	17	.4	3	.1
Answers with comment	143	3.1	205	4.4

In the questionnaires sent to the ministers the following question was asked: "Please describe in as detailed form as you desire the most significant thing you or your religious institution have done during the past year for social justice." A general tabulation of the answers of 2,000 of the religious leaders to this question is as follows:

TABLE XII. CONCRETE EXAMPLES OF INDIVIDUAL AND GROUP ACTION

	No. of Activities	Per cent
Forums, discussions, conferences, institutes: race relations, war, disarmament, military training, world court, peace, coöperative movement, economic justice, anti-Semitism, munitions, leisure time, utilities, Russia, unemployment, movies.....	442	28.6

[2] It should be understood that the writer is not implying in any of the above treatment that the ministers should have replied in one way as over against another. He is merely tabulating the returns.

TABLE XII (*Continued*).

	No. of Activities	Per cent
Preaching with emphasis on personal salvation	314	20.3
Acts of charity: poor relief, clothing and feeding the unemployed, Red Cross, opening church for unemployed, etc.	203	13.1
Educational activity: teaching, writing articles and books, advancing ideas of technocracy, putting on elaborate programs of education, studying social literature, taking courses, opposing the closing of schools, carrying on adult education under F.E.R.A., putting books into libraries, putting on dramas with social and economic subjects, conducting social insurance study groups, Parent-Teachers work, denouncing economy of scarcity, research working for international understanding, circulating literature, handing out tracts, putting up posters, publishing pronouncements, getting up resolutions	176	11.4
Aggressive radical social action: working to raise wage standards, preventing evictions and foreclosures, fighting against discrimination in relief, breaking up political corruption, helping to defeat red-baiting candidates, anti-Nazi work, defense of share croppers, keeping out fly-by-night sweat shops, cleaning up city government, housing, socialization of hospitals, standing for law enforcement, interviewing civic authorities, running for office on Socialist ticket, inviting Socialist speakers, preached on street, serving on local social action committees, opposing Father Coughlin	80	5.2
Preaching with definite social meaning	78	5.0
Temperance activity	59	3.7
Supporting national movements for social justice: Socialist party, Epic plan, N.U.S.J., L.I.D., N.R.L.F., A.C.L.U., League for Social Justice, N.A.A.C.P., F.O.R.	47	3.0
Constructive action in relation to unemployment: surveying conditions, organizing the unemployed, setting up meetings, formulating statements of policy and working for legislation; helping secure jobs, conducting free unemployment agency	45	2.9
Social service: helping juvenile delinquents and young people on probation, assisting jail farm projects, working for community gardens, aiding mental defectives, hospitalization, public health, helping build community hall, service in transient bureau or camp, scout work, anti-crime activity	26	1.7
Religious action through inter-faith and ministerial groups: inducing ministers to work on social creed, organizing community church, setting up inter-faith meetings, serving on special denominational social action committees, relating church federations to local community affairs	24	1.4
Personnel work and student counseling	22	1.4
Coöperation with the government: members of government relief boards, administration of relief, coöperation on crop control, chairman or member Compliance Board	16	1.0
Religious Devotion: Prayer, Oxford group	7	.5
Miscellaneous	8	.5

It will be seen from the above that one-fifth of all the clergymen who responded believe that preaching the individual gospel, as they often put it "of Jesus Christ and him crucified," is enough. This is social action to them and other more aggressive forms are extraneous

to the gospel pattern. About a quarter have taken action through forums, discussions and conferences, while 13 per cent have felt that in engaging in charity work they have done their part.

A study of the above tabulation will show that the Protestant minister to some extent tends to veer away from action in proportion as it is against accepted patterns of behavior involving conflict with the status quo or with property power. Sociologically this is what would be expected. On the other hand, there is a vigorous minority which does take radical action in conflict with the established customs or the vested interests of our time. For instance one minister in Greensboro, N.C. cites the following: "The Greensboro Ministerial Association, on my motion, requested the sheriff of Guilford County to disarm his deputized, mill-paid gunmen during the general textile strike, September, 1934. We were the only group of ministers in the state to speak on this issue. This request so publicized the practice of using gunmen that the governor denied implication in the arming of undisciplined men and rebuked the sheriff for so implicating him." A minister in Chester, Pa., wrote: "I have made repeated charges publicly, from pulpit, in Rotary, and Kiwanis meetings, in the presence of Ford officials, concerning the slavery in the Chester plant; and concerning the unfair methods of Ford in 'dumping' the unemployed into Detroit and refusing to care for them; and similar atrocities."

Conclusion

On the whole the religious leader, just the same as any other individual, is profoundly influenced by the social milieu in which he is immersed. In general he probably does not take much more radical action than the overwhelming majority of the respectable class in his community. On the other hand, the intentions of the minister for social justice are higher than the average of his community and this makes for tension between the ideal aims of the minister and his practical and successful functioning in the local community. A minority of the clergy satisfy this tension by radical action which is at variance with the pattern of the herd. A majority seek their outlets and satisfactions in the main in religious work, which does not involve conflict with community standards.

EDITORIAL

To Members of the American Sociological Society:

The American Sociological Society was founded in 1905. At the end of its first year it had a membership of 115. During the past thirty years the membership has increased over ten-fold; sociology as an academic discipline has been instituted in hundreds of colleges and universities where it was either unknown or feared as dangerous to the sacred traditions of American ideology. Meanwhile, the sociological field has broadened and the character of sociological literature profoundly altered. Without losing altogether the traits of social philosophy on the one hand, or of a sympathetic study of socially maladjusted types of individuals on the other, sociology has become a field of exacting research, utilizing the most effective techniques yet developed in the social sciences. Thus both in years and in character of effort our society may be said to have come of age.

This process of maturation has been greatly facilitated by several sociological journals, of which this new Review becomes, in a sense, a competitor. Their services have been and still are invaluable. We do not consider the Review to be in any degree designed to displace them. Rather, we aim simply to fill the obvious need for enlarged avenues of publication and for the definite instrument of organic expression required by our maturity as a Society, the remarkable growth of sociological personnel, and the mounting output of constructive thought and research. We hereby extend our greetings and best wishes to these other journals, and express our sincere hope and belief that they will continue in the future, as in the past, to be indispensable to all members of the guild.

It is the desire of the Editorial Board to make the Review of the utmost value to all members of the Society. We view it as essentially a coöperative undertaking, in which all are participants. To a very great extent the quality of the Review will depend on the helpful interest taken in its contents by all individual members and all regional groups. We look upon it as one of the most important working tools of American sociologists. It will endeavor faithfully to reflect their creative thinking, the research activities, and the participation in practical affairs. It can do this, however, only as they supply it with the essential materials. Obviously it is impossible for us to attain our best standard of editorial work in the first issues. We shall doubtless make mistakes and commit offenses; for these we ask your tolerant good will. At all times constructive suggestions for improvements and alterations will be most welcome.

In addition to the leading articles, we plan to publish in each number extensive bibliographic materials, including book reviews, lists of new books, and references to periodicals. In this field we hope to give considerable attention to foreign contributions. Hence we shall try to secure the assistance of foreign correspondents, as well as to find among our own members those willing to supply systematically data concerning important foreign books, periodicals, personages, and societies. In all these and other matters we depend on your interested coöperation. The REVIEW belongs to you collectively and should be made to express your interests.

THE EDITORIAL BOARD

EDITORIAL NOTES

THIS FIRST NUMBER of the REVIEW must be looked upon as more of a promise than a fulfillment. It has seemed to the Editor better to bring it out as near as possible to the normal date of publication, February 15, rather than to strive for greater completeness at the expense of delay. Some months will elapse before efficient coöperation can be established among the numerous persons involved in the enterprise. As noted above, the Editor conceives the REVIEW as an important working tool of American sociologists; we hope to enlist the active participation of as many workers as possible.

To this end it will be of great help if those who would like to do book reviews would send in their names to the Editorial Office with indication of the subject of their greatest interest.

We also request that items of personal and departmental interest be sent to the office without special solicitation. We suggest that the larger departments of sociology appoint one of their members to supply this information more or less regularly.

During the four-months absence of the Editor on research in Germany (Feb. 12 to early June), the other members of the Editorial Board will make all decisions which cannot be deferred or dealt with on the basis of precedent. Professor Howard Becker of Smith College will serve as Acting Editor during this period, and will be responsible, in conjunction with the Editorial Board, for the routine of the second issue. The third issue will be edited by President Henry Pratt Fairchild, who has kindly consented to assume the burden, and the Acting Editor.

We extend our greetings to the various regional societies and their officers. This country is so large and our membership so numerous that there is abundant room for their activities. Moreover, these activities are evidences of vitality, increasing strength and confidence, and the emergence of greater scientific realism among the profession. It would be a pity, however, if the regional societies should come to think of themselves as in any active sense substitutes for, or rivals of, the parent organization. It will be the policy of the REVIEW, therefore, to encourage the use of its pages, so far as space permits, for publicity regarding their activities, through the publication of the programs of sessions and of papers there presented.

Persons *and* Positions

Ph.D. Columbia; married; ten years of teaching in professorial rank; five years of governmental service; experience in social work and business; world-wide traveler; extensive research work; author of many books; seeks chair in leading university, college or research institution.

B.S. in Economics with first-class honors; Ph.D. in Sociology; studied London School of Economics and Political Science, Harvard, Chicago and California Universities; assistant in Sociology, University of London 1926–28, in Social Biology, University College, London, 1929–31; Rockefeller fellowship, United States, 1931–33; Institute of Sociology and Institute of Social Research, London, 1933–35; author of books and articles. Seeks teaching position in an American college or university.

Official Reports *and* Proceedings

THE AMERICAN SOCIOLOGICAL SOCIETY

ANNUAL REPORT OF THE SECRETARY FOR THE FISCAL YEAR, DECEMBER 15, 1934 TO DECEMBER 14, 1935

Membership Statement

On December 15, the number of members of the American Sociological Society was 1,164, or a loss of 38 from the 1934 figure.

Membership in 1934		1,202
Members resigning	29	
Members dropped	288	
Members deceased	3	
Total lost		320
Honorary members	7	
Life members	29	
Members renewing	914	
New members (of whom 119 are students)	214	1,164

Student Membership

Through recommendations by their instructors, over a hundred students took advantage in 1935 of the special rate for student membership in the Society. Attention is called to the coöperation of the following persons who sent in names of students recommended for membership: Theodore F. Abel, Harry Alpert, C. Arnold Anderson, W. A. Anderson, Read Bain, Belle Boone Beard, L. L. Bernard, W. S. Bittner, Herbert Blumer, Emory S. Bogardus, Ernest W. Burgess, C. J. Bushnell, W. F. Byron, M. G. Caldwell, F. Stuart Chapin, Carroll D. Clark, Lloyd A. Cook, L. S. Cottrell, Jr., C. A. Ellwood, Arthur S. Emig, Earle Eubank, H. P. Fairchild, Ellsworth Faris, Eleanor J. Flynn, Clarence Glick, N. S. Hayner, Joseph Husslein, Carl Kelsey, Paul H. Landis, H. D. Lasswell, Porter R. Lee, J. P. Lichtenberger, A. W. Lind, Mary J. McCormick, R. D. McKenzie, R. W. Murchie, W. F. Ogburn, A. S. Radcliffe, J. A. Rademaker, E. B. Reuter, Dwight Sanderson, Calvin F. Schmid, Edward Shils, T. Lynn Smith, F. F. Stephan, W. B. Stone, Edwin H. Sutherland, F. M. Thrasher, W. Russell Tylor, J. H. Useem, E. W. Voelker, Willard Waller, Walter T. Watson, R. Clyde White, Louis Wirth, Carle C. Zimmerman.

Activities of the Society which are carried on in coöperation with other

social sciences are reported upon by our representatives on national organizations such as the Social Science Research Council, the American Council of Learned Societies, the *Encyclopaedia of the Social Sciences*, the *Dictionary of American Biography*, the *American Year Book*, and *Social Studies*.

Special attention should be given to the reports of three committees of the Society for the current year—the Research Planning Committee, the Committee on Opportunities for Trained Sociologists, and the Committee on Publications.

Invitations to the 1936 Meeting

Invitations for our next annual meeting have been received from Chicago, Detroit, Indianapolis, Montreal, Nashville, New Orleans, Pittsburgh, St. Petersburg, and Toronto.

Necrology

The Secretary regrets to report the death of the following members in the past year: Kenyon L. Butterfield, Hannah B. Clark Powell, and Frank Hatch Streightoff.

Respectfully submitted,
HERBERT BLUMER, *Secretary*

ANNUAL REPORT OF THE MANAGING EDITOR OF THE *PUBLICATION* FOR THE FISCAL YEAR

DECEMBER 15, 1934, TO DECEMBER 15, 1935

On December 15, the volumes of *Papers and Proceedings* on hand were as follows:

Volume	Copies	Volume	Copies
I	25	XVI	16
II	0 (out of print)	XVII	33
III	0 (out of print)	XVIII	16
IV	0 (out of print)	XIX	188
V	0 (out of print)	XX	70
VI	0 (out of print)	XXI	236
VII	0 (out of print)	XXII	99
VIII	19	XXIII	104
IX	0 (out of print)	XXIV	323
X	51	XXV	348
XI	0 (out of print)	XXVI	100
XII	20	XXVII	324
XIII	4	XXVIII	152
XIV	0 (out of print)	XXIX	211
XV	150		

The total number of volumes, 2,489, is 123 more than reported last year.

Respectfully submitted,
HERBERT BLUMER, *Managing Editor*

REPORT OF THE FINANCE COMMITTEE

The statement of Cash Receipts and Disbursements for the fiscal year shows an excess of bills unpaid on December 15, 1935, of $1,713.20, over and above cash on hand and credit. This amount, if added to the 1936 income of $133.75 and to the sum of $155.62, the uninvested receipts from Life Memberships, gives the figure $2,002.57, which represents the accumulated deficit of the Society. If this total deficit be subtracted from the deficit of $2,121.73, as it stood at the end of the previous fiscal year, the resultant sum of $119.16 constitutes the surplus in operations for the current year.

In accordance with instructions given by the Executive Committee in its action on December 27, 1934, the Finance Committee formulated early in the year and put into operation a plan for the retirement of the accumulated deficit of the Society. This plan took the form of a sale to members, of non-interest-bearing Certificates of Indebtedness, in $10.00 denominations. These certificates were issued in order of purchase, with the agreement that they would be retired serially, at the rate of twenty per year, beginning with the 1936 fiscal year. At the end of the 1935 fiscal year, 124 of these certificates had been purchased, enabling the Society to reduce the interest-bearing debt to the sum of $706.93. It is strongly urged that action be taken at once to sell the remaining certificates, in order that the Society may effect an economy in the interest charge now made on its debt.

ARTHUR J. TODD, *Chairman*
E. W. BURGESS
EARLE EUBANK
M. J. KARPF
E. D. TETREAU
R. CLYDE WHITE

BUSINESS MEETING OF THE AMERICAN SOCIOLOGICAL SOCIETY

DECEMBER 27, 1935, 9:15 A.M.

Meeting called to order at 9:15 A.M., President Chapin presiding.

E. A. Ross presented the report as representative of the Society to the American Council of Learned Societies.

L. L. Bernard presented the report as representative of the Society to the *American Year Book*.

The Secretary read a letter from Robert S. Lynd as representative of the Society to the American Library Association.

F. Stuart Chapin presented the report as representative of the Society to the American Association for the Advancement of Science.

In the absence of the official delegate of the Society to the Social Science Research Council, Robert Crane, Executive Secretary of the Social Science Research Council, spoke informally about the work of the Council.

E. George Payne presented the report as representative of the Society to *Social Studies*.

H. P. Fairchild presented the report as representative of the Society to the *Dictionary of American Biography*.

W. P. Meroney read a series of proposed amendments to the By-laws of the constitution of the Society, reading as follows:

The following amendments to the By-Laws of the American Sociological Society are hereby offered:

It is moved that the following sections be added to Article III, Committees:

Sec. 13. The Editorial Board of the Society shall be composed of the President and Secretary of the Society, an Editor and a Managing Editor, and six Associate Editors elected by the Society for three-year terms, two of which shall expire each year.

Sec. 14. The Editorial Board shall meet upon the call of its Chairman, or of a majority of its members.

Sec. 15. The Editorial Board shall have specific responsibility for the publication of the AMERICAN SOCIOLOGICAL REVIEW, the official journal of the Society, or of any other publications ordered by the Society. In carrying out these responsibilities it may appoint such subcommittees and Associate and Special-Issue Editors, and may associate with it such other members of the Society, as the administration of these responsibilities may require.

Sec. 16. The President and Secretary of the Society shall be respectively ex-officio Chairman and Secretary of the Editorial Board. In the absence of either or both, the Editorial Board shall elect a Chairman or Secretary pro tem. In event of a vacancy occurring on the Editorial Board, it shall be filled by the Editorial Board until the next annual meeting of the Society.

Meeting adjourned at 9:38 A.M.

Respectfully submitted,
HERBERT BLUMER, *Secretary*

MEETING OF THE EXECUTIVE COMMITTEE

DECEMBER 27, 1935
COMMODORE HOTEL, ROOM F

Meeting called to order at 5:10 P.M., by President Chapin. Members present: E. A. Ross, Robert E. Park, F. Stuart Chapin, Arthur J. Todd, William I. Thomas, William F. Ogburn, L. L. Bernard, E. B. Reuter, Jerome Davis, W. P. Meroney, J. H. S. Bossard, and J. O. Hertzler.

It was voted to dispense with the reading of the minutes of last year's meetings of the Executive Committee, since they appear in published form in Volume 29, No. 1, of the *Publication* of the Society.

The Secretary presented his annual report. On motion of William F. Ogburn, it was voted to accept the report.

The Managing Editor of the *Publication* presented his annual report. On motion of Arthur J. Todd, it was voted to accept the report.

The report of the Finance Committee was presented by Arthur J. Todd, chairman. Jerome Davis moved that the report be filed. Seconded by L. L. Bernard. Motion carried.

The report of the Committee on Honorary Members was presented by Earle E. Eubank, proposing G. L. Duprat and Franz Oppenheimer for honorary membership. On motion of Mr. Davis, it was voted to recommend to the Society the adoption of the report. Motion carried.

President Chapin presented the report of the Research Planning Committee. On motion of W. P. Meroney, seconded by Jerome Davis, it was voted to accept the report.

The report of the Committee on Publications was presented by W. P. Meroney, chairman. Mr. Meroney moved that the Executive Committee recommend to the Society the adoption of the report. Seconded by Jerome Davis. Motion failed to carry.

Meeting adjourned, on motion by Jerome Davis, at 6:42 P.M.

Respectfully submitted,
HERBERT BLUMER, *Secretary*

BUSINESS MEETING OF THE SOCIETY

DECEMBER 28, 1935
COMMODORE HOTEL, EAST BALLROOM

The minutes of the meeting of the Executive Committee were read by the Secretary.

The report of the Committee on Honorary Members was presented by Earle E. Eubank, chairman, proposing the election to honorary membership of Dr. Franz Oppenheimer and Dr. G. L. Duprat. On motion by Mr. Eubank, seconded by Mr. Sorokin, the recommendation of the Committee was adopted.

The report of the Research Planning Committee was presented by F. Stuart Chapin, chairman.

The report of the Committee on Publications, specifying the following seventeen recommendations, was presented by W. P. Meroney, chairman. Mr. Meroney moved the adoption of the report, with the voting by ballot. Seconded by Mr. Davis. Motion carried, 78 to 42.

Howard Becker moved that the President select a special nominating committee to nominate candidates for the Editorial Board. Motion carried.

Meeting adjourned at 10:30 A.M.

Respectfully submitted,
HERBERT BLUMER, *Secretary*

REPORT OF THE COMMITTEE ON PUBLICATION OF THE AMERICAN SOCIOLOGICAL SOCIETY[1]

The Committee on Publications of the American Sociological Society was first appointed by Dr. L. L. Bernard, then president, acting under direct instructions from the floor at the business meeting of the Society in December 1932. It was charged with the study of the situation as then

[1] The complete report was published in the *Publication* of the Society for December 1935. The "Recommendations" are here republished because of minor verbal changes.

existing in the Society with regard to the various journals of sociology and with the formulation of a policy with respect to the separate Publications of the Society.

Based upon a report of this Committee in December 1933, and upon the recommendations of Dr. Ellsworth Faris, Editor, the Society voted to discontinue the *American Journal of Sociology* as the official organ of the Society, effective December 1934. It also voted that the journal subscription included in the annual dues of the Society, on the option of the individual member, might be applied to any of the major journals of sociology.

At the meeting of the Society in 1934, notice was given to the University of Chicago Press that its contract with the Society for the printing of the Publication of the Society would be terminated in December 1935.

In view of these facts, it is obvious that the Society must take some steps at the forthcoming New York meeting in December looking to a future arrangement regarding its publications. The committee has made diligent study of the entire matter over a period of three years and wishes to submit herewith a summary of its findings in advance of the New York meeting in order that time may be given for serious consideration. The final report will be submitted to the first meeting of the Executive Committee in New York and, in turn, with its recommendations, to the business meeting of the Society. It will be more detailed and will attempt to cover as many aspects of the problem as possible and to anticipate questions which may arise at that time. During the interim, the Committee will welcome criticism and suggestions from any member of the Society. It is our wish that the most constructive thought of the Society be mobilized and brought into genuine agreement and hearty accord in regard to the issues involved in this report.

Recommendations

The Committee recommends as follows:

1. That the name Publication of the American Sociological Society, as at present used, be changed to the American Sociological Review, with a subtitle The Official Journal of the American Sociological Society.

2. That the Publication of the American Sociological Society with the change in name be expanded into a bi-monthly journal of sociology, to conform in general to the specifications outlined separately and herewith attached, as may be determined by the Editorial Board.

3. That an Editor and a Managing Editor be elected by the Society to serve for two years; that six Assistant Editors be elected, two to serve one year, two to serve two years, and two to serve three years; and that the society hereafter elect each year two assistant editors to serve for three years and every other year an editor and a managing editor each to serve for two years.

4. That the eight elected editors with the President and Secretary of the Society shall constitute an Editorial Board which shall have complete control and management of the American Sociological Review and any other

publications of the Society not otherwise provided for; and that the Editor, Managing Editor and the President of the Society shall constitute the Executive Committee of the Editorial Board.

5. That the Editorial Board, in accordance with the above changes, be instructed and authorized to secure from the Post-Office Department of the United States the proper re-entry as Second Class mail matter at the post-office of the printing company with which the contract for publication of the REVIEW may be made.

6. That four dollars ($4.00) of each of the annual regular membership dues and three dollars and fifty cents ($3.50) of each of the student membership dues to the Society be set aside as a publication fund subject to the direction of the Editorial Board to be used for the publication, editorial, and secretarial expenses of the AMERICAN SOCIOLOGICAL REVIEW and, when adequate, for the expenses of any other publications ordered by the Society.

7. That the present practice of including subscriptions to other sociological journals in the annual dues of the Society be discontinued; but that the Secretary of the Society be instructed to receive and transmit subscriptions to other standard sociological journals at such reduced contract rates as they may offer.

8. That the Editor shall have jurisdiction over the editorial management and policies of the REVIEW subject to the limitations of paragraph 10; that he shall prepare or approve all copy for publication; that he shall have the authority to appoint such associate, contributing, or special-issue editors as he may deem necessary; and that he shall exercise such other responsibilities and perform such other duties as are usually incumbent upon such officer.

9. That the Managing Editor shall have jurisdiction over and attend to the business details of the REVIEW, such as: contracts for publication, soliciting and contracting advertisements, subscriptions outside the regular membership, exchanges, etc.; that all expenses for his services shall be met from the income from advertisements; that he shall be bonded for the faithful handling of all funds to an amount to be determined by the Editorial Board.

10. That the final jurisdiction in matters of general editorial policy or of business management shall rest with the Editorial Board, which shall have the power to reverse the decisions either of the Editor or of the Managing Editor by a majority vote.

11. That the net income from advertisements and subscriptions be added to the publication fund; that, should there remain a surplus in this fund after all publication, secretarial and editorial expenses have been met, any such surplus may be applied by the Executive Committee of the Society to the employment either of a full time Editor or Managing Editor or to payment for editorial services.

12. That the Editorial Board be instructed to keep all financial expenditures for all phases of the AMERICAN SOCIOLOGICAL REVIEW within the income from the part of the dues allocated for this purpose, the income from subscriptions, and the net income from advertisements, and to incur no

financial obligations that in their best judgment cannot be met from these sources.

13. That all expenses for stenographic assistance, postage, and other necessary outlays indispensable to the editorial direction and business management of the REVIEW shall be met by the Society out of the publication fund; provided that no traveling or other special and unusual expense shall be incurred without first being approved by the Executive Committee of the American Sociological Society; that, in the event this fund is insufficient to meet all such imperative and necessary needs, the Executive Committee of the Society be authorized to make a special appropriation from the other available funds of the Society for this purpose.

14. That, pending the establishment of the REVIEW upon a financial basis that will permit the remuneration of the Editors for their services, those elected be requested to serve for two years without pay other than for their actual and necessary expenses; and that the Editorial Board distribute the editorial burden under the direction of the Editor, by departmental arrangements and special issues covering the major divisions and sections of the Society, so that the burden need not be unduly irksome to anyone.

15. That it be recognized by the Society that the acceptance of a place on the program of an annual meeting, both general and divisional, shall confer first publication rights on the Society for the paper as presented and obligate the author to prepare the paper in a form suitable for publication; that this publication right, on the request of the author, may be waived by the Editor; and that the Editor shall have the right to reject for publication any paper which fails to measure up to the standards required either in content or form.

16. That the Society recognize at this time the need of adopting a broad general publication policy and to that end instruct the Board of Editors to devise ways and means as early as possible to establish a series of monograph publications to be issued under the auspices of the Society.

17. That the Society also instruct the Board of Editors to establish as early as possible a series of abstracts of current sociological literature and research subjects and that, pending such an independent arrangement, space be provided for this purpose in the REVIEW.

MEETING OF THE EXECUTIVE COMMITTEE

DECEMBER 28, 1935
COMMODORE HOTEL, ROOM F

Meeting called to order by President Chapin, at 5:20 P.M. Members present: F. Stuart Chapin, William I. Thomas, James Q. Dealey, William F. Ogburn, John L. Gillin, E. B. Reuter, Arthur J. Todd, Jerome Davis, J. H. S. Bossard, J. O. Hertzler.

The report of the Committee on Opportunities for Trained Sociologists was presented by Walter C. Reckless, chairman. The report is printed in Volume 29, No. 4, of the *Publication* of the Society. Acting on the recommendations of the Committee, the Executive Committee voted—

1. That Mr. Chapin be designated as the liaison man between the Committee and the Carnegie Corporation in carrying on negotiations for the securing of a grant.

2. That a special committee on Public School Relations be established, with Charles C. Peters as chairman.

3. That the President appoint a committee to continue with the tasks of the Committee on Opportunities. Mr. Chapin appointed as the Committee, Walter C. Reckless, chairman; Joseph Mayer, Wilson Gee, C. C. Peters.

The report of the Committee to Solicit an Endowment for Annual Awards was presented by Arthur J. Todd, chairman. On motion by Mr. Todd, seconded by Mr. Gillin, it was voted to discontinue the Committee.

Earle Eubank presented a request for the study of affiliation with the International Federation of Sociological Societies and Institutes. On motion of Mr. Davis, seconded by Mr. Todd, it was voted—

> That a committee of three be appointed by the President of the Society to study the question of an affiliation of the American Sociological Society with the International Federation of Sociological Societies and Institutes, sponsored by the International Institute of Sociology at Geneva, and to bring in a report at the 1936 annual meeting of this Society.

On motion of Mr. Todd, seconded by Mr. Davis, it was voted that the special nominating committee for the editorial staff be authorized to propose nominees and post names.

The Secretary read a petition signed by members of the Society for approval of the formation of an autonomous section of the Society on Political Sociology. On motion of Mr. Thomas, seconded by Jerome Davis, it was voted to recommend to the Society the approval of the formation of this section.

Harold A. Phelps was elected Secretary-Treasurer of the Society.

On motion by Mr. Reuter, seconded by Mr. Gillin, Mr. William F. Ogburn was selected as the Society's representative to the Social Science Research Council for a three year term.

On motion by Mr. Meroney, seconded by Mr. Bossard, it was voted to elect E. W. Burgess to the Research Planning Committee for a three year term.

On motion by Mr. Gillin, it was voted to recommend to the Society that the new President be empowered to appoint the representative of the Society to the American Association for the Advancement of Science.

On motion by Mr. Davis, it was voted that the Society discontinue its representation on the *American Year Book*, but that Mr. Bernard be requested to continue to prepare his annual review of sociology for publication in the AMERICAN SOCIOLOGICAL REVIEW, if approved by the Editor and Managing Editor.

Mr. Fairchild was reappointed as the Society's representative to the *Dictionary of American Biography*.

On motion by Mr. Gillin, seconded by Mr. Bossard, it was voted to select

Joseph Mayer as the Society's representative to the American Library Association.

Meeting adjourned at 6:43 P.M.

Respectfully submitted,
HERBERT BLUMER, *Secretary*

ANNUAL BUSINESS MEETING OF THE SOCIETY

DECEMBER 30, 1935
COMMODORE HOTEL, EAST BALLROOM

Meeting called to order by President Chapin, at 9:10 A.M.

The recommendations of the Committee on Opportunities were presented to the Society. The recommendations were—

a. That Mr. Chapin be designated as the liaison man between the Committee and the Carnegie Corporation in carrying on negotiations for the securing of a grant.
b. That a special committee on public school relations be established, with Charles C. Peters as chairman.
c. That the President appoint a committee to continue with the tasks of the Committee on Opportunities. Mr. Chapin appointed as the Committee, Walter C. Reckless, chairman; Joseph Mayer, Wilson Gee, and C. C. Peters.

On motion of Mr. Sorokin it was voted to adopt the recommendations and to approve the action of the Executive Committee.

On motion of Mr. Lumley, seconded by Mr. Gillin, it was voted to approve the action of the Executive Committee in having the new President appoint a committee to study the question of affiliation with the International Federation of Sociological Societies and Institutes.

On motion by Mr. Sorokin, it was voted to approve the action of the Executive Committee in sanctioning the formation of an autonomous section on Political Sociology.

On motion by Earle Eubank, seconded by E. A. Ross, it was voted to approve the election of Harold A. Phelps as Secretary-Treasurer.

On motion by Howard Becker, it was voted to give a rising vote of thanks to the outgoing Secretary.

On motion of Franklin Johnson, it was voted to approve the selection of William F. Ogburn as the Society's representative to the Social Science Research Council for a three year term.

On motion by Mr. Ross, it was voted to approve the recommendation of the Executive Committee to entrust the new President with power to appoint the Society's representative to the American Association for the Advancement of Science.

On motion by Mr. Gillin, seconded by Mr. Lumley, it was voted to approve the selection of Mr. H. P. Fairchild as the Society's representative to the *Dictionary of American Biography*.

On motion by Mr. Sorokin, it was voted to approve the selection of Mr.

Joseph Mayer as the Society's representative to the American Library Association.

On motion by Mr. Davis, it was voted to take from the table the by-laws presented to the Society by Mr. Meroney at the Business Meeting on December 27.

On motion of Mr. Meroney, it was voted to incorporate the following amendments into the constitution of the Society:

Sec. 13. The Editorial Board of the Society shall be composed of the President and Secretary of the Society, an Editor and a Managing Editor, elected by the Society for two-year terms, and six additional members elected by the Society for three-year terms, two of which shall expire each year.

Sec. 14. The Editorial Board shall meet upon the call of its Chairman, or of a majority of its members.

Sec. 15. The Editorial Board shall have specific responsibility for the publication of the AMERICAN SOCIOLOGICAL REVIEW, the official journal of the Society, or of any other publications ordered by the Society. In carrying out these responsibilities it may appoint such subcommittees and Associate and Special-Issue Editors, and may associate with it such other members of the Society as the administration of these responsibilities may require.

Sec. 16. The President and Secretary of the Society shall be respectively ex-officio Chairman and Secretary of the Editorial Board. In the absence of either or both, the Editorial Board shall elect a Chairman or Secretary pro tem. In event of a vacancy occurring on the Editorial Board, it shall be filled by the Editorial Board until the next annual meeting of the Society.

On motion of Jerome Davis, seconded by E. A. Ross, it was voted to express appreciation to the Editors of the *American Journal of Sociology* for the service rendered the Society in the past.

The report of the Committee on Nominations was presented by E. B. Reuter, chairman. The following nominees were proposed:

For President—

STUART A. RICE
HENRY P. FAIRCHILD

For First Vice-President—

DWIGHT SANDERSON
F. E. LUMLEY

For Second Vice-President—

J. H. KOLB
M. C. ELMER

For Executive Committee—

DOROTHY S. THOMAS
GEORGE A. LUNDBERG
JESSE F. STEINER
JEAN DAVIS

Mr. Stuart Rice withdrew his name for consideration for President.

Mr. R. M. MacIver was nominated from the floor by Mr. P. A. Sorokin, for President.

The following were elected:

President, H. P. FAIRCHILD
First Vice-President, DWIGHT SANDERSON
Second Vice-President, J. H. KOLB
Members of the Executive Committee, DOROTHY S. THOMAS and JESSE F. STEINER

Mr. Howard Becker, chairman of the special committee on Nominations, presented the following slate of nominees for the Editorial Board of the AMERICAN SOCIOLOGICAL REVIEW:

For Editor—

FRANK H. HANKINS
GEORGE A. LUNDBERG

For Managing Editor—

HAROLD A. PHELPS
W. P. MERONEY

For six members of the Editorial Board—

THEODORE F. ABEL
L. L. BERNARD
JEROME DAVIS
C. A. ELLWOOD
J. K. FOLSOM
C. E. GEHLKE
J. L. GILLIN
J. O. HERTZLER
J. H. KOLB
F. E. LUMLEY
W. C. RECKLESS
C. C. ZIMMERMAN

Mr. George Lundberg withdrew his name from consideration for Editor. On motion of Mr. Gill, the Secretary was instructed to cast the unanimous ballot for Mr. F. H. Hankins for Editor.

Mr. Harold A. Phelps was elected Managing Editor.

The following were nominated from the floor for the Editorial Board:

NEVA DEARDORFF
CLIFFORD KIRKPATRICK
TALCOTT PARSONS

The following were elected members of the Editorial Board:

J. K. FOLSOM—Three Years
JEROME DAVIS—Three Years
L. L. BERNARD—Two Years
NEVA DEARDORFF—Two Years
JOHN L. GILLIN—One Year
C. E. GEHLKE—One Year

The report of the Committee on Resolutions was presented by E. H. Sutherland, chairman, as follows:

Be it Resolved, That the American Sociological Society express its appreciation and thanks for the efficient and comfortable entertainment of this annual meeting, as provided by the Committee on Local Arrangements, of which Professor Frederic M. Thrasher was chairman, and by the Commodore Hotel.

Be it Resolved, that the American Sociological Society express its appreciation of the efficient services of the retiring officers. The Society is particularly indebted to Dr. Herbert Blumer, who has carried ably and effectively the heavy responsibilities of the secretaryship during the past four years, and to Miss Lelia Smith, who has been Secretarial Assistant in the Chicago office since 1923, and whose wholehearted and unstinted services have contributed much to the success of the Society.

On motion by Mr. C. C. North, it was voted to accept the report of the Committee on Resolutions.

Mr. North moved that it was the sense of the membership of the Society that it was undesirable, in arranging the dates for the annual meeting, to have it extend over more than three days. Seconded by Miss Susan M. Kingsbury. Motion failed to carry.

Mr. Scott Nearing moved that it was the sense of the membership that in determining the general theme of the program for the next annual meeting, efforts be taken to have it focus on some phase of the present social crisis. Motion carried.

Meeting adjourned at 10:35 A.M.

Respectfully submitted,
HERBERT BLUMER, *Secretary*

FINANCIAL STATEMENT

SCHEDULE "A"

BALANCE SHEET AS OF DECEMBER 15, 1935

Assets

Cash in bank ($469.90 held for investment fund)		$ 703.63
Inventory:		
2,489 volumes of *Proceedings* at 50 cents		1,245.50
Securities (at book value):		
Northwestern Electric Company 6 per cent Gold Bonds	$ 500.00	
Hyde Park Baptist Church House 6 per cent Gold Bonds	600.00	
American Telephone and Telegraph Company Common Stock	296.00	1,396.00
Office Furniture and Fixtures	$ 32.63	
Less depreciation—up to and including 1935	3.26	29.37
Total assets		$3,374.50

Liabilities

Accounts payable		$1,946.93
Net worth:		
Surplus, balance December 16, 1934	$2,611.27	
Less loss for period December 15, 1934, to December 15, 1935	1,183.70	1,427.57
		3,374.50

SCHEDULE "B"

STATEMENT OF RECEIPTS AND EXPENDITURES FROM DECEMBER 15, 1934, TO DECEMBER 15, 1935

Cash on deposit and credit on bills on December 16, 1934		$1,679.58	
Cash Receipts			
Dues and subscriptions from members (for 1935)	$5,305.54		
Dues and subscriptions from members for 1936	133.75		
Certificates of Indebtedness	1,240.00		
Exchange	9.25		
Postage	6.70		
Proceedings, book royalties	741.53		
Interest on Investments	93.00		
Manual of Abstracts	72.75		
Membership List	45.53		
Special contributions	106.58	7,754.63	
Total receipts			$9,434.21
Expenditures			
Subscriptions:			
American Journal of Sociology		$2,496.38	
Social Forces		622.88	
Journal of Educational Sociology		68.92	
Sociology and Social Research		144.67	$3,332.85
Publication of the Society			1,249.54
Printing			177.48
Clerical aid and salaries			1,725.19
Honorarium to Secretary			500.00
Postage and Express			265.05
Stationery			139.85
American Council of Learned Societies			35.00
Exchange			43.93
Membership refunds			25.30
Auditing			10.00
Manual of Abstracts			63.70
Annual Meeting			10.00
Miscellaneous (of which $1,240—sale of Certificates of Indebtedness; $40.84 interest on debt)			1,349.61
Office Expense			39.96
Total expenditures (for 1935)			$ 8,966.46
Cash disbursements on unpaid bills (1934)			1,566.34
Total cash disbursements			$10,532.80
Bills outstanding		$1,946.93	
Cash payments		8,585.87	
		$10,532.80	
Bills outstanding on December 15, 1935			$1,946.93
Total cash on hand			233.73
Excess of bills over cash on hand			$1,713.20
Cash on deposit, December 15, 1934			1,091.08
Net loss for year			$2,804.28

ANALYSIS OF ACTUAL INCOMES AND EXPENDITURES

1917–1935

	Receipts from Dues	Total Receipts	Expenditures	Deficit	Cash Balance
1917					$ 380.65
1918	$2,415.35	$2,810.70	$ 2,803.87	$ 53.13	327.48
1919	2,598.30	2,962.79	3,196.74	235.95	93.53
1920	3,172.50	3,591.96	3,815.90	233.94	−130.41
1921	3,708.50	4,400.73	4,617.22	216.49	−346.90
1922	4,228.72	4,093.79	5,002.75	98.96	−445.86
1923[1]	4,439.45	5,097.86	4,994.08	103.78[2]	−342.08
1924[1]	4,722.40	5,516.78	5,328.68	188.10[2]	−153.98
1925[1]	4,332.84	5,223.17	5,444.36	213.19	−367.17
1926[1]	4,382.00	5,982.62	5,820.50	162.12[2]	−205.05
1927[1]	5,221.85	6,417.46	6,811.98	394.52	−599.57
1928[1]	6,275.62	7,912.54	7,783.65	128.89	−470.68
1929[1]	7,258.05	8,769.79	8,818.95	49.16	−519.84
1930	7,547.96	9,459.43	10,335.15	875.72	−1,395.56
1931	7,609.02	9,408.87	10,478.41	1,069.24	−2,465.20
1932	7,412.14	8,898.25	9,081.90	183.65	−2,648.85
1933	6,250.90	7,644.62	7,400.06	277.13[2]	−2,371.72
1934	6,407.15	7,404.73	7,154.74	249.99[2]	−2,121.73
1935	6,801.39	7,876.73	7,726.46	119.16[2]	−2,002.57

[1] Receipts from life memberships are not included.
[2] Surplus.

CURRENT ITEMS

Rural Sociology, a new journal to be published quarterly, is being established this year by the Section on Rural Sociology of the American Sociological Society. The managing editor of this journal, which is to be published by The Louisiana State University Press, is Professor T. Lynn Smith of Louisiana State University. The editors are: Dr. Lowry Nelson, director of the Utah Agricultural Experiment Station (now on leave with the Rural Resettlement Division, Washington, D.C.); John H. Kolb of the University of Wisconsin; C. E. Lively of Ohio State University; Dwight Sanderson of Cornell University; and Carle C. Zimmerman of Harvard University. The price is $2.00 a year. This magazine is subsidized by Louisiana State University for the promotion of research and the propagation of more exact knowledge of rural life. The only qualification which the publishers have put upon the editors is that the magazine must attempt to establish itself as quickly as possible as an authoritative expression of the best thought in the field of rural sociology. Carefully prepared and thoughtful manuscripts in this field should be sent to Dr. T. Lynn Smith, Louisiana State University, Baton Rouge. Subscriptions should be sent to the same address. The price is kept low so that libraries, graduate students, and others interested in public affairs may subscribe. In each state, a committee is to be appointed which will see that the teachers in rural sociology, the libraries, the research agencies, and the graduate students are given an opportunity to become subscribers to the journal.

Eastern Sociological Conference. The annual meeting of the Eastern Sociological Conference will be held on April 18 and 19 in New Haven, Connecticut. The central topic of the Conference will be *Social Theory and the Social Order*. The following sections and papers are being arranged: "Social Economics and the Social Order": Talcott Parsons, chairman; papers by Talcott Parsons and Max Lerner; "Social Psychology and the Social Order": Joseph K. Folsom, chairman; papers by Frankwood Williams, Gardner Murphy, and Edward Sapir; "Political Science and the Social Order": R. M. MacIver, chairman; papers by Hans Speier and R. M. MacIver; "Revolution and Evolution in Social Change": Leland H. Jenks, chairman; papers by Lyford Edwards and Roger Baldwin. At the banquet meeting there will be a paper on "History and the Social Order" by Harry Elmer Barnes, and the presidential address on "Sociology and the Problem of the Social Order" by James W. Woodard.

A second series of meetings contains the following sections: "Fascism as a Social Order": Theodore Abel, chairman; papers by Lowell Field and

Theodore Abel; "Capitalism as a Social Order": Howard Becker, chairman; papers by B. A. Anderson, Jr. and Ernest G. Draper; "Communism and Socialism as Social Orders"; Jerome Davis and H. P. Fairchild, co-chairmen; papers by Scott Nearing and Sidney Hook, Harry Laidler and H. A. Miller.

A third series of sections contains the following: "Religion in Relation to the Social Order": Ray Abrams, chairman; papers by Jerome Davis, Arthur Swift, and Ray Abrams; "Social Work and the Social Order": Hugh Carter, chairman; papers by Esther L. Brown, Maurice Taylor and Mary Van Kleeck; "Education and the Social Order": E. George Payne, chairman; papers by W. H. Kilpatrick, Nathaniel Peffer.

One round table for report on research projects will be held under the chairmanship of George E. Simpson; and another on the nature of sociology and the nature and training of the sociologist, based on the report of the following committee: W. Rex Crawford, Chairman, Howard Becker and Henry Pratt Fairchild.

Attention of all sociologists in the Northeastern area is called to these meetings; and they are invited to attend and to affiliate themselves with the Conference. Students of adjacent fields with sociological interests are also invited to attend these meetings.

Ohio Sociological Society. The annual Spring meeting of the Ohio Sociological Society will be held in Columbus, Ohio, April 24 and 25, 1936. The Society, founded in 1925, now has the active participation of over two-thirds of the more than ninety sociologists in the forty-odd colleges and universities of the state. The officers for the current year, who are planning the annual program, are: President, James A. Quinn, University of Cincinnati; Vice-President, C. B. Gohdes, Capital University; Secretary-Treasurer, S. C. Newman, Ohio State University; Editor of *The Ohio Sociologist*, F. E. Lumley, Ohio State University.

Pacific Sociological Society. The Pacific Sociological Society held its Seventh Conference, together with Coast Economists, at Mills College, Oakland, California, December 27–28. Forty members from four states were in attendance; greetings were received from Hawaii. Papers read will be summarized in a forthcoming issue of *Sociology and Social Research*, and in the *Proceedings*. Prof. Charles Reynolds of Stanford University was elected President, and Prof. Martin H. Neumeyer of the University of Southern California was re-elected Secretary-Treasurer. At the business session considerable attention was given to the question of more effective and equable representation in the activities of the American Sociological Society and related matters. An Advisory Committee, with Prof. Emory Bogardus as Chairman, was appointed to consider plans for improved organization, apportionment of dues, coördination of journals, and integration of national and regional programs.

The program included the following topics and papers. *Social and Political Methods and Objectives*, Walter S. Hertzog, chairman: "Pareto and Social Objectives," by Emory S. Bogardus; "Dictatorship," by Herbert B. Alexander; "Analysis of Fascism," by Francis Wilson. *Social Security*,

Clement A. Kerman, chairman: "Individual Family and Social Security," by Mary G. Luck; "Social Security," by Jesse F. Steiner; "A Physician and Social Insurance," by Dr. T. H. Kelley. *Social Objectives*, Richard T. La Piere, chairman: "Objectives in Social Evolution," by David E. Henely; "Social Objectives in Race Relations," by Floyd C. Covington; "The New Leisure and Social Objectives," by Martin H. Neumeyer. *The Functions of Sociologists in Social Reconstruction*, E. H. Moore, chairman: "Sociological Research and Political Leadership," by C. W. Topping; "Sociological Indoctrination under Conditions of Dictatorship," by Carl M. Dent. At a dinner session Dr. William Kirk spoke on "Social Objectives of Maori Culture"; at luncheon Dr. Carl E. Carlson spoke on "Social Objectives of Relief Programs." The presidential address by Dr. Howard B. Woolston was on "The American Intellectual and Social Reform" (to be published in the June issue of the AMERICAN SOCIOLOGICAL REVIEW).

Political Sociology. At the Annual Meeting in New York, an autonomous section on Political Sociology was established. Dr. Maurice Parmelee, who has been serving as an economist with the A.A.A., was elected its Chairman, and Professor C. R. Hoffer of Michigan State College, its Secretary. A program of at least two sessions is contemplated for this section at the 1936 Annual Meeting.

Anglo-American Sociological Conference. Mr. Jay Rumney, who is now in America on a Rockefeller fellowship, informs us that the Institute of Sociology in London is planning a conference in London in 1937, prior to the conference to be held at Paris in connection with the World Exposition.

Southern Sociological Society. The Southern Sociological Society will hold its annual meeting at Atlanta, Georgia, April 17–18. The officers of the Society are: E. T. Kreuger, Vanderbilt University, President; Wilson Gee, University of Virginia, First Vice-President and Chairman, Committee on Research; Comer M. Woodward, Emory University, Second Vice-President; Rupert B. Vance, University of North Carolina, Secretary-Treasurer; H. C. Brearly, Clemson College, Chairman, Membership Committee; and R. F. Bellamy, Florida State College, Chairman, Committee on Resolutions. Elected members of the Executive Committee are: L. M. Bristol, University of Florida; N. B. Bond, University of Mississippi; E. W. Gregory, University of Alabama; C. S. Johnson, Fisk University; E. W. Montgomery, University of Kentucky; and B. O. Williams, Clemson College.

The program will include the following sections and papers: Section on *Social Welfare and Social Policy*, G. Croft Williams, chairman: "Emerging Problems in Public Welfare," by Coyle E. Moore; "Public Welfare Objectives of the Federal Government," by Alan Johnstone; "Coöperation of the States with the Federal Government in Public Welfare," unassigned. Section on *Race and Culture*, E. W. Gregory, Jr., chairman: "A Research Project in Race and Culture," by Guy B. Johnson; "White Culture Segregations in Louisiana," by Harlan W. Gilmore; "Methodology in Studies of Race and Culture," by Floyd N. House. Section on *Regional Studies and Planning*, Howard W. Odum, chairman: "A Sociological Critique of Re-

cent Southern Regional Studies," by Wayland J. Hayes; "Regional Planning as Applied to the Southeast," by Rupert B. Vance; "Theoretical and Research Implications of Regionalism," by Howard W. Odum. Section on *The Teaching of Sociology*, L. M. Bristol, chairman: "Aims in the Teaching of Sociology," by H. E. Jensen; "Principles of Education and the Teaching of Sociology," by D. G. Stout; "Correlation of Social Sciences in High School and College—the North Carolina Plan," by Harold D. Meyer; "The Social Sciences in the Yonge Laboratory School, University of Florida," by Louis Guissez. Section on *Rural Life and Problems*, C. Horace Hamilton, chairman: "Types of Farming in Land Tenure," by Linden S. Dodson; "Racial Factors in Land Tenure," by Monroe Work; "Social Mobility and the Land Tenure Problem," by B. O. Williams. There will be a report of the Committee on Social Research in the South, Wilson Gee, chairman; and a luncheon discussion of "Will the Bankhead-Jones Act Solve the Land Tenure Problem?", C. Horace Hamilton, chairman, and T. Lynn Smith, Rupert B. Vance and Charles P. Loomis, discussants.

University of Arizona. In September 1935, Dr. E. D. Tetreau, formerly analyst with the Federal Emergency Relief Administration and Associate Professor of Rural Sociology at Ohio State University, accepted a position at the University of Arizona as Rural Sociologist on The Experiment Station staff. He began his duties October 1st.

University of Louisiana. Dr. Harold F. Hoffsommer, formerly of Alabama Polytechnic Institute, has been appointed Assistant Professor of Sociology at Louisiana State University. He will assume his duties on March 1, 1936. Professor Carle C. Zimmerman of Harvard University will teach courses during the coming summer session.

University of Michigan. The Institute of Health and Social Sciences, at Detroit, an integral part of the Graduate School of the University, announces for the second term, beginning in February 1936, courses in social engineering, social research, mental measurement, nature and varieties of human behavior, psychiatry, and various aspects of social work. Prof. Robert W. Kelso is Chairman and Prof. R. D. McKenzie, Special Adviser in Sociology.

The New York School of Social Work published in its *Bulletin* for October 1935 account of "An Experiment in Providing Instruction for Relief Workers," by Miss M. Antoinette Cannow. The experiment was carried out at Dallas, Texas, in coöperation with the Social Service Division of the Texas Relief Commission.

Oberlin College. Dr. Newell LeRoy Sims, Chairman of the Department of Sociology, has returned to the United States after more than six months of travel in Europe with his wife and daughter. He will be located in Washington, D. C., for several months, engaged in research, and will resume his duties at Oberlin in the Autumn. During Professor Sims' sabbatical year, Prof. F. M. Zorbaugh is acting chairman of the department, and Dr. Edwin Harvey is visiting professor.

Professor Sims has just completed a three-year term on the Executive Committee of the American Sociological Society, and was Chairman of the

Society's Committee on Publications, being especially active in connection with the first report of that Committee.

University of Pittsburgh. Dr. David K. Bruner, formerly of the University of Pennsylvania, has been added to the staff of the Division of Social Work of the University of Pittsburgh. Dr. Bruner will have charge of the courses in Public Welfare Administration and Community Organization.

Smith College. Prof. F. H. Hankins has received a stipend from the Oberlaender Trust of the Carl Schurz Foundation. He plans to sail on February 13 for four months' study of population trends in Germany. His address will be: Carl Schurz Memorial Foundation, Berlin. Prof. Howard Becker, who during the academic year 1934–35 was engaged in research in Europe, was on leave for the first semester in order to give courses on Culture and Conduct and Social Thought before Comte at Harvard University. Prof. Margaret Marsh plans to spend sabbatical leave in travel in the Orient this spring and summer.

University of Washington. The staff of the Department of Sociology numbers nine, and the total student registration, 1200, of whom some 200 are majoring in this field. The Professional School of Social Work, established under the guidance of the Department, has fifty graduate students, mostly from northwestern states. Prof. Walter G. Beach of Stanford University has taken over the organization of extension work in Seattle and throughout the State.

Washington University. The Department of Sociology at Washington University has been expanded into the Department of Sociology and Anthropology. Courses in Physical and Cultural Anthropology, Primitive Religion and Magic, Folkways and Folklore, Race, Archaeology, and Cultural Contacts are now listed. Six instructors are giving part of their time to these new courses. Prof. Walter D. Bodenhafer spent part of the summer of 1935 at Harvard University preparing for the course in Cultural Anthropology. Doctors L. L. and Jessie Bernard spent August and September in Paris on a grant from the Social Science Research Council. The purpose of this study was to determine the relationship between the French Positivist Societies and similar societies in the United States during the period 1851 to the present. Dr. Jessie Bernard continued the investigation in Paris through December and in England during December, January, and February.

Yale University. The first sociology course ever given in a university was opened by Prof. William G. Sumner in 1875. Its diamond jubilee was observed last year. At the same time the Yale University Press announced the definitive edition of Sumner's essays. Prof. A. G. Keller, who has spent some thirty years in the study of the growth of social institutions, is putting some of the fruits of his reflections into a series of essays on aspects of the modern world. Prof. George P. Murdock has spent the last two summers, 1934 and 1935, in anthropological study of the Tenino Indians of Oregon. He has been assisted by Mr. John Whiting. Recent books from the Yale Department include: *Frontier Folkways*, by James G. Leyburn (Yale Univ. Press, $3.00); and *Insurance or Dole*, by E. Wight Bakké

(Yale Univ. Press, $2.50). Student registrations in Sociology have grown rapidly in recent years. In 1935 the total in the six undergraduate courses was 569, of whom sixty-one were majoring in the subject.

The American Birth Control League held its Fifteenth Annual Meeting in New York City, January 22–23, 1936. Among the addresses of general interest were : "Population and Peace," by Prof. Stephen Duggan, Director, Institute of International Education; and "The Individual and Social Change," by Helen M. Harris, Head Worker, Union Settlement, New York City.

The National Urban League announces its annual competitive examination for Fellowships in social work for colored students. Applicants must be graduates of, or candidates for graduation from, accredited colleges. Successful candidates will receive tuition and monthly stipends valued together at approximately $1,000 for the school year.

Applications must be filed before March 1, 1936, on forms furnished by the National Urban League, which may be secured by writing to T. Arnold Hill, Acting Executive Secretary, 1133 Broadway, room 826, New York City, N.Y. The examination will be held later in March.

BOOK REVIEWS

HOWARD BECKER
Assistant to the Editor

The Mind and Society. By VILFREDO PARETO, translated and edited by Andrew Bongiorno and Arthur Livingston. New York: Harcourt, Brace and Co., 1935. 4 vols, 2033 pp. $20.00.

The American discussion of Pareto's treatise on general sociology[1] has already attained considerable dimensions, but to one fairly careful follower of it at least the impression the secondary literature gives is one of considerable confusion. Almost the only way in which the lines can be drawn is in terms of pro- and anti-. I can see little sign of a clarification of the substantive issues. In view of this situation it does not seem profitable to enter into a polemical discussion with any of the interpreters. I shall rather devote the present discussion to exposition of what seem to me to be some of the central elements of Pareto's thought and an indication of their derivation. I shall not attempt any general critical evaluation.

Some of Pareto's strong partisans have claimed for him an extraordinary degree of detachment from the currents of social thought of his time, implying perhaps that it was quite futile to try to understand him in terms of the theories and problems of his immediate predecessors. To a certain degree this is true. In a narrowly technical sense he had no immediate predecessors in sociology; he was member of no recognized "school." On the other hand, in a broader sense the principal components which go to make up his sociological theory are all elements which have played a major role in modern thought about man and society. His thought is part of, and deeply influenced by, the great broad stream of which sociology in a technical sense is a surface eddy.

We may single out three currents of this stream as of particular importance in the present context. One is the methodology of positive science, particularly the physical sciences. The second is the conception of rational action, particularly as developed in the main line of "orthodox" economic theory. The third, finally, is less definite but nevertheless highly important: it is the "humanistic" tradition of knowledge of the history and literature of the ancient world. The particular way in which Pareto combined these three elements will go a long way toward explaining the character of his work. The first two are most important for his formal analytical scheme,

[1] References here will be to the numbered sections which are uniform throughout the three editions.

the last for the more empirical aspects of his work, including his trends of choice in illustrative material.

The part which has been played by the methodological model of the physical sciences in all the social disciplines since about the eighteenth century is surely of paramount importance, not least in sociology. Directly or indirectly almost all the methodological discussions have turned on this group of questions, and a very large number of writers have set out to make sociology a science according to this model. In this respect Pareto's enterprise has a long list of antecedents. The interpretation of what the methodological model of physical science implied in direct relation to social data has, however, varied considerably. Here Pareto differs quite markedly from his most influential predecessors, such as Spencer.

By no means everything is completely clarified in Pareto's general methodological position. I cannot, in this brief discussion, enter into a careful analysis of the various problems involved. The following, however, seems to me the *main* line of his methodological doctrine, the one which fits in best with his substantive theoretical structure. *At a number of points the trend singled out is in conflict with others in his thought*, but for the sake of brevity I shall ignore this fact.

Science, Pareto says, is "logico-experimental."[2] This may be taken to mean that in a scientific theory are admissible two and only two orders of elements: logically correct reasoning and statements of observed or observable fact. The antithesis of the logical element is fallacious reasoning, which causes no methodological but only practical difficulties. Everyone is agreed that scientific theory is bound by the norms of logical correctness. It is in connection with the other, the element of experimental fact, that the methodological difficulties arise; there is by no means agreement as to what constitutes observable fact.

One thing seems to be clear, namely, the universal requirement that a fact shall not be "subjective," that it shall be as it were forced on the scientist whether he will or no, and thus be independent of his wishes or "personal equation." In this sense Pareto continually contrasts it with a "manifestation of a sentiment." It is also something which is observable and verifiable. A proposition is only a statement of fact, so far as its content is capable of submission to cognitive experience, experimentally or otherwise. At least potential agreement in their descriptive terms by different observers is certainly a requirement of the concept of fact.

Beyond this it is noteworthy that Pareto does not lay down any rigid criteria as to what constitutes fact. He does not use the term "sense impression," or any related term, from which one could infer a "materialistic" position. His whole conception, with the two exceptions of the basic distinctions in principle between facts and manifestations of sentiments on the one hand, and metaphysical, that is, unverifiable propositions on the other, remains highly flexible and undogmatic. Even here it is only the distinction in principle on which he insists. The concrete point at which the line is to be drawn remains open to discussion.

[2] The principal methodological discussion is to be found in Chapter I.

Two other points, however, are noteworthy. Pareto's failure to limit facts to sense data is important because he evidently does not share a view which is often associated with that formula, in the extreme case by the behaviorists. That is, he does not exclude data concerning the subjective "states of mind" of persons other than the scientific observer from the category of fact. On the contrary, as we shall see, the "subjective aspect" of human action is central to his whole scheme. In particular, it seems to me from his treatment of "theories and propositions" as observable facts[3] it is quite legitimate to infer that not only the physical properties of symbols, linguistic and otherwise, are included in the status of fact, but also their meanings. It is true that Pareto does not subject this question to explicit methodological analysis. But two things may be said. His explicit methodology leaves the question open and his actual scientific work involves a very large amount of this kind of observation.

On the other question he is more explicit. A fact, while it must be observable, need not be a complete concrete phenomenon. It may be an element or an aspect of a concrete phenomenon. Above all it is not quantitative completeness of factual knowledge which is the aim of science and the measure of its achievement. Indeed, Pareto holds it is strictly impossible to know "all the facts" about any given phenomenon, and were it possible it would be undesirable.[4] Pareto's insistence on the role of fact in science is not a repudiation of theoretical abstraction. On the contrary, the element of abstraction is involved in his concept of fact itself. A fact is a theoretically significant observable aspect, element, or property of a concrete phenomenon.

Similarly, a scientific theory is not a mere aggregation of discrete statements of fact. It is a statement of logically *interrelated* facts. It involves "laws," which Pareto defines as "uniformities in the facts,"[5] or as we may say, uniform modes of relation between facts. It is in the concept of law that the significance of abstraction comes out most clearly. A concrete phenomenon is in general a meeting point of the "operation" of a number of laws.[6] It is quite illegitimate to require, as a test of validity, direct concrete correspondence between the expectations derived from a scientific law and the concrete course of events. This will exist only so far as the latter is free from the influence of elements not formulated in the law in question. The logical necessity inherent in scientific theory must not be translated into empirical necessity.

This brings us to the second of the main elements of Pareto's thought we are considering. Scientific theory would certainly be normally held to constitute a "rational" achievement of the human mind. A body of thought deeply preoccupied at the same time both with the status of scientific theory and with human action could not fail to attempt to bring the two together. That is, there is raised the question: In what sense and to what extent could human action be understood in terms of the scientific knowl-

[3] Sec. 7, 18, 81.
[4] See especially Sec. 33, 39.
[5] Sec. 99.
[6] *Ibid.*

edge of his situation possessed or capable of acquisition by the actor? This problem has played a very great part indeed in the development of modern social thought.

The point at which approximately this standard of rationality of action has undergone the most systematic theoretical elaboration in a social science is in orthodox economic theory. The fact that Pareto was at the same time trained in the physical sciences and deeply preoccupied with scientific methodology, *plus* the fact that he was an eminent economic theorist, gives the setting for his analytical scheme. It must not be forgotten that the methodology of science is important not merely as guiding his attempt to remain strictly logico-experimental in his sociological procedure, but also it provides the criteria for one of the main *substantive* distinctions of his own positive theory.

I took pains above to state Pareto's view that scientific facts are not necessarily fully concrete facts but contain an element of abstraction. This view is of direct substantive importance in the present connection. Specifically, Pareto's principal motive for embarking on his sociological studies was his conviction of the abstractness of economic theory. That is, he saw very clearly that the theoretical concepts of economics were inadequate to the full understanding of certain concrete phenomena in which an economic element was unquestionably involved, such as the effects of a protective tariff. From this inadequacy he did not, however, conclude with the "institutionalists" that the economic theory was wrong, but only that it was in need of supplementing by other theories which, synthesized with the economic, would give the required understanding.[7] He found, however, that other theories of this character which satisfied him were not to be met with elsewhere and so set out to build them up himself. Hence the treatise.

When he came to formulate the economic element in relation to the others he felt to be in need of analysis, it became evident that what was dealt with explicitly in economics shared with certain other elements of action this dominant property of rationality according to a scientific standard. This circumstance led him to make his basic distinction not between economic and non-economic action, but between the broader category of "economic plus"[8] which he called "logical" action and the non-logical.

This fact threw the emphasis on defining the category of logical action away from the criteria specific to the economic upon those common to it and the other "logical" elements. This was the element of rationality according to the "scientific" standard. Thus Pareto defines logical action as action consisting of "operations logically united to their end" from the point of view both of the actor himself and of an observer with a "more extended knowledge of the circumstances."[9] It involves, we may say, a purposive adaptation to the exigencies of the situation which is subjectively intended. At the same time the "correctness" of the actor's subjective

[7] Sec. 34.
[8] See Sec. 152 for an enumeration of the content of the "plus."
[9] Sec. 150.

"theory" is verified by the more extended knowledge of the observer, that is, by the best available scientific knowledge.

It is essential to note certain things about the distinction between logical and non-logical action. In the first place it is, as Pareto explicitly says, not a classification of *kinds of* action but of *elements in* action.[10] A concrete act is neither logical nor non-logical but always involves both elements. It is logical, as Pareto puts it, *so far as* it may be regarded as resulting from "a process of reasoning," that is, a scientifically verifiable "theory" of the facts and conditions of the situation in which the actor is placed. This does not imply that *any* concrete act is exclusively understandable in such terms. Even though, as I think is the case, Pareto does not consistently adhere to this position, it is the only basis on which his scheme as a whole makes sense.

Secondly, only the category of logical action is positively defined at all, leaving the non-logical as a *residual category*. It is action so far as it is *not* understandable in terms of the logical element, and that is all. It is absolutely essential to keep this fact in mind if one is to understand the subsequent development of Pareto's theory.

Finally, it may be noted that the line between logical and non-logical is drawn in subjective terms, in terms of the character of the "theory," the "process of reasoning," which the actor associates with his act. On the basis of observation of the external course of events alone the distinction cannot be drawn, for, so far as it is scientifically understandable, the result necessarily follows from the "operations" performed.

Having made this basic distinction, Pareto leaves logical action aside and proceeds to the explicit study of the non-logical. He does this by a peculiar procedure which it is important to understand correctly. He first makes a rough empirical distinction between three orders of elements involved in non-logical action. There are two kinds of empirically observable facts roughly distinguishable as overt acts and the linguistic expressions or "theories" associated with them. These he denominates as B and C respectively.[11] Both these stand in a functional relation of mutual interdependence with a third entity A, the "state of mind" of the actor. This state of mind is not positively defined at all, but is simply the "subjective" locus of the non-logical elements of action. So far as the external environment is rationally apprehended, its influence on concrete action will be embodied in the scientifically verifiable "theories," that is, in the logical element, and hence is of no interest in the present analysis. Hence it is in this state of mind that some at least of the main determinants of non-logical action are to be found. Pareto more generally refers to it by the blanket residual term "sentiment." It is not directly observed in the same sense as B and C, but through its "manifestations" in them.

Both B and C may legitimately be studied as means of throwing light on A. But Pareto deliberately chooses to neglect B and confine his analytical attention to C. An overt act is always to a large extent an adaptation to an

[10] Sec. 148.
[11] Sec. 162.

external situation. This, however, is not to the same extent true of a "theory" which is more directly an "expression" of the state of mind of its proponent. This is the principal reason for the choice. But whatever others may be involved the choice has an important consequence, that of bringing the standard of scientific validity back into the center of the stage again. For, if logical action is distinguished by the conformity of the theories associated with it to this standard, it follows that departure from the standard, for whatever reason, is, as far as *theories* are concerned, the criterion of the non-logical. Pareto's procedure then is an inductive study of theories *so far as* they depart from the scientific standard.[12]

The standard itself, as Pareto formulates it, admits of two types of departure: fallacies in reasoning on the one hand, and the inclusion in the place of statements of fact of elements which rest on erroneous observation, or which refer to non-empirical, non-verifiable entities, on the other. The inductive study leads to a discrimination between a central, relatively constant core of such theories, and a contingent, much more highly variable element. The former is always analogous to the statement of fact in a scientific theory. But the theories in question, being by definition non-scientific, always depart from this. Pareto employs the general formula that it is the "manifestation of certain sentiments." It is to be remembered that this element is an element of a theory, that is, is a proposition. The other, the variable element, may depart from the scientific standard both by including manifestations of sentiments in place of other statements of fact and by involving logical fallacies. The constant element is the residue, the variable the derivation.[13] This is the way in which Pareto arrived at the categories which have formed the central focus of the discussion of his work.

In interpreting what they mean, let us remember that non-logical action in the first place was a residual category and that this character is shared by the corresponding non-scientific theories. The residue is nothing but the inductively determined constant element in these theories. It is not and cannot be the basic explanatory category of a developed theory of non-logical action. It is, as Pareto remarked, analogous to a thermometer reading.[14] The "causes" lie not in the residue but in what it manifests. Though it is true Pareto does not in practice adhere strictly to this concept of the residue, it is the only definition for which sanction may be found in his work.

The residue is always a manifestation of sentiments. Hence the real problem of interpretation shifts to this category. In view of the original residual character of the category of non-logical action, it is not surprising that sentiment should turn out on further analysis not to be a homogeneous category. Pareto himself never carried the explicit analysis further. It is, however, possible by careful study of other parts of his own work[15] to discern the emergence of certain further distinctions. There is no space

[12] Occupying principally Chapters IV and V.

[13] (a) and (b) respectively which are elements of the non-scientific theories (c). See Sec. 803. This is not to be confused with the A, B, C, referred to above.

[14] Sec. 875.

[15] Especially the discussion of Social Utility in Chapter XII.

here to present the evidence for this view, but I should like to present dogmatically two fundamental distinctions which are, I think, indispensable to further progress with Pareto's mode of approach.

In the first place, it is necessary to discriminate those factors which may be summed up as "heredity and environment," which influence human action in modes other than the rational adaptation involved in logical action, from a quite distinct category which may be called "value" elements. Of the former category the most conspicuous elements are those involved in what may loosely be called "instinct," those hereditary tendencies of behavior which are relatively independent of conscious ratiocination. It is this group which has predominantly occupied our own anti-rationalist psychologists and sociologists, and significantly enough the great majority of interpreters have jumped to the conclusion that this is the real burden of Pareto's thought. It is *one* element but stands by no means alone. It is, incidentally, as should be quite evident, not the residue, but one of the things *manifested in* the residues. An instinct is not a proposition.

Pareto's original approach through the concept of rationality of action as used in economics inevitably brought the categories of means and ends and their relations into the center of attention. But it turns out that his concept of logical action concerns only the character of the means-end relationship;[16] action is logical so far as operations, i.e., means, are logically united to their end. But every system of means-end relationships will logically involve a system of ultimate ends which must, so far as they constitute causally relevant elements of action, according to Pareto's own position, be non-logical. But, as Pareto's own later work shows,[17] this element is not reducible to terms of heredity and environment or any psychological concept of instinct. Ultimate ends and the value-sentiments from which they are derived constitute an independent category within that of sentiment in general.

The second emergent distinction is on a different level. The concept of logical action involves the conception of the orientation of action to a certain type of norm, which may be called a "norm of efficiency." This is that of the most "appropriate" adaptation of means to an end[18] according to an intrinsic, scientifically demonstrable standard. It is possible for action to deviate from a norm of this character not merely in one respect but in two. On the one hand, the relevant norm may be of this "efficiency" type, but the actions in fact to a greater or less degree fail to attain it. That is, knowledge of the norm the actor professes to be striving to attain is in so far irrelevant to the understanding of his action. This is certainly the case with which the anti-intellectualist psychologists have predominantly been concerned. It is the one most frequently imputed to Pareto. It is one, but only one, of the things with which he was concerned.

[16] There is no space to present the justification for this and the following statements here. See my article "The Place of Ultimate Values in Sociological Theory," *International Journal of Ethics*, April, 1935.

[17] Especially his treatment of social utility, Chapter XIII.

[18] Sec. 150.

On the other hand, the deviation may concern the character of the normative elements themselves. Under this heading come two types of cases. In the first place, so far as an ultimate end is involved which is not to the actor part of a scientific theory but is the manifestation of a sentiment, a non-logical element is present. But this order of non-logical elements is not by itself a factor of deviation from the norm; it is rather a way of stating that the total concrete norm is not definable in terms of logical action alone. Secondly, the operations may not be "logically" united to their ends but non-logically, or arbitrarily. This is a case of great empirical prominence in Pareto's work, that of ritual actions. In discussing ritual, there is no question of a failure of the overt acts to correspond to the theory; that is taken for granted. It is the non-scientific character of the theory which is the non-logical element. *In both these cases there is no warrant whatever for the assumption that the sentiments manifested in these kinds of non-logicality are the instincts of psychology.* On the contrary, there is every reason to believe that value elements play a major part.

Enough has been said, I think, to show that Pareto's residue and derivation analysis does not constitute a developed analytical framework for sociological theory. It is rather an approach to the problem of building up such a theory. Indeed, Pareto himself, implicitly at least, went several steps beyond it in this direction. Sociology is not, however, a science so highly developed on its theoretical side as to permit such incompleteness, even though it be serious, to be made an excuse for rejecting Pareto's attempt out of hand as useless. On the contrary, I should like to draw particular attention to two features of Pareto's scheme which seem to me to make it extremely useful. (1) It approaches the problem of a general theory of human action in society through the problem of rationality in a relatively clearly definable sense. This sense is not one arbitrarily invented by himself, but has been thoroughly tested in a great part of modern social thought and is the foundation of what most would agree is, in systematic theoretical thinking, the most highly developed of the social sciences, namely, economics. But unlike most economists who have concentrated their attention upon this element for its own sake, Pareto in his sociology makes it the point of departure for a theory of the other elements in explicit relation to this. It is the explicit relation to the norm of economic rationality which has been lacking in most previous attempts, especially by psychologists, to deal with what Pareto calls the non-logical elements of action. Pareto was very far from completing the job, and in some directions more than he achieved is to be found in the work of other writers. But he defined this point of departure more clearly than any other writer of whom I know. Surely this is *one* of the most fruitful approaches to the major theoretical problems of sociology.

(2) The residue-derivation analysis seems to me to contain a technique of the greatest value which has not been so adequately developed elsewhere. The objectivist trend of our thought has tended to turn us away from the use of "speech reactions" as a field of data for study, except where their relation to overt action is of the simple and obvious kind involved in

Pareto's concept of logical action. Pareto has, I think, shown the possibility of dealing with "theories" in the non-logical context in such a way as to avoid the usual rationalistic fallacies and yet extract significant conclusions from the data. This same result has been achieved on a grand scale by psychoanalysis, but with a bias derived from preoccupation with neurotic symptoms. Pareto has gone a considerable distance in developing a technique for studying action through the medium of linguistic manifestations on the social level and with interest centered on the normal, while psychoanalysis has been primarily concerned with the particular individual and the abnormal.

If the above view of the incompleteness of Pareto's analytical scheme is correct, then we should expect that it failed to give a full explanation of his empirical sociological theories, his classification of the residues, his account of cycles of social change and of other social processes such as that of "crystallization." I think it does account for certain broad lines of his empirical views, but by no means all of them. This would imply that there is an empirical element in Pareto's work for which we have not yet accounted, which cannot be reduced to terms of any well-defined analytical categories.

It is unlikely that this would be found to have any one source. But one certainly stands out with particular prominence. Pareto was throughout his life an avid student of the history and literature of classical antiquity. His sociological treatise is, as every reader of it knows, heavily weighted with illustrative material drawn from this source. Along with it he absorbed a set of attitudes and opinions which may be called "aristocratic humanism." In such terms he built up the outline of a philosophy of history, the most striking expression of which is to be found in his cyclical theory of social change.[19] Within the broad lines of which I have spoken, this theory seems to me to be derived from empirical sources and his reading of ancient authors and certain semi-moderns, among whom Machiavelli stands out. Its connection with the analytical scheme to which most of our attention has been devoted is relatively loose. Certainly one who tries to work with the analytical scheme is not bound by the details of the cyclical theory.

The principal connecting link between these two aspects of his work seems to me to be the classification of the residues. So far as I know no one has ever subjected this classification to a thorough critical analysis with the analytical scheme and its further development in mind. My general impression is, however, that the classification is less a product of Pareto's analytical thinking than it is of the empirical historical aspect of his work. It is more akin to a classification of minerals according to their easily describable properties such as color, weight, etc., than according to chemical composition.

This empirical element entering into the classification of the residues, as distinct from the technical concept residue, seems to be one of the things

[19] Developed in Chapter XII.

which accounts for Pareto's confusing lack of consistency in the use of the term. For in his practical use of the first two classes of residues, the "instinct of combinations" and the "persistence of aggregates," he seems to be speaking not of classes of propositions involved in non-scientific theories, but of general tendencies of action involving both overt behavior of certain types and *total* theories of certain types, e.g. scepticism and faith. This situation is definitely confusing, and has lent color to the interpretation of the residue as "a fancy name for instinct." After all, the term instinct has fluctuated in meaning between a name for a hereditary *element in* action and for a concrete kind or tendency of action. Pareto's inconsistency seems to me more an index of the incompleteness of his analytical theory than of anything else.

TALCOTT PARSONS

Harvard University

Primitive Law. By A. S. DIAMOND, M.A., L.L.M., Barrister-at-Law of Gray's Inn and the North-Eastern Circuit. New York: Longmans, Green and Co., 1935. Pp. x+451. $10.00.

This book is intended to demonstrate the falsity of Sir Henry Maine's theories which are expounded in his *Ancient Law* (first American edition 1864) and in his *Dissertations on Early Law and Custom* (1883). Since both books embrace many subjects of sociology and early social history, their criticism will be of interest to readers of this *Review*.

It is not the first time that Maine's hypotheses have been attacked. Anthropologists who have at their disposal the material obtained since Maine's epoch have long since alienated themselves from his opinions. Nevertheless, his ideas still hold sway in the minds of these jurists and sociologists who are not conversant with the results of research in other fields. Diamond's venture is, therefore, beneficial.

The author divides his book into three parts. He first considers a number of sources, chiefly records of old written laws such as the Babylonian Code of Hammurabi, the Assyrian laws and the Hittite Code. The second part is intended to criticize Maine's view that early law was determined by religion and morals. The Anglo-Saxon Laws, the Pentateuch, the Hebrew Code, the Code of Manu, and the Fragments of the Twelve Tables are included to substantiate the author's contention. In the third part, the author discusses the development of courts and legislation and such institutions as marriage, inheritance and property. Here are treated crime and civil injury, procedure, ordeal and oath, and last of all, contract.

Diamond argues that law refers to the rules of conduct between man and man, while religion refers to those between man and nature. In my opinion, the pattern of thinking determines law as well as religion. The author denies (p. 197) that law originated from the practice of voluntary arbitration, that arbitration can therefore be regarded as the precursor of courts, and that the customary law, derived from the decisions rendered in the

voluntary arbitrations, was later incorporated into the ancient codes (p. 206). Primitive law implies an order without which no community can exist; it does not need for its foundation the previous decisions of arbitrators. Those judgments, on the contrary, are outward manifestations of (what I call) "latent" laws, laws which are not defined by express words, but which exist subconsciously and which are applied when necessary. In this I agree with Diamond. I agree with him also in his opposition to the common belief that "primitive man regards law as unchangeable" (p. 205). And I believe that he is correct in refuting Maine's allegation that ancient law is dominated by technicalities and formalism which override the importance of the rules (p. 350). Ordeal and oath do not quite "come from outside," as Diamond remarks, but seem to be an expression of "magical thought," which is itself the outcome of a mentality responsible for the character of primitive law. Maine's idea that the growth of the law of contract is due to the love of formalism is one of his queerest fancies. It is without basis, and is, of course, disputed by our author.

It is time to examine Maine's constructions in the light of new knowledge acquired in the last seventy years. The author has, on the whole, succeeded in proving his contentions, although he refrains from supplanting the worn-out constructions with new ones. He repeatedly refers to a background of psychological and governmental influences which shape social conditions and eventually determine law. But he does not establish sharp-cut relations. This is due chiefly to the inadequacy of material with regard to primitive institutions, their cultural foundation, economic features, political processes, etc. It is impossible to get a satisfactory insight into the mechanism involved by merely relying, as does the author, on a schematic survey such as Hobhouse-Wheeler-Ginsberg's book provides. Whatever may be the merit of that scheme, it is a tentative abstraction from a host of various facts and is meant as a perspicuous summary for the student of the concrete phenomena. The forces which shaped, transformed and pushed forward primitive and ancient law can only be investigated by a study of the concrete communities. Had the author proceeded in such a manner, he would have evaded the danger of a sweeping evolutionism which hardly fits into his criticism of Maine. He should not have called his book "Primitive Law," as it deals mainly with ancient law exposed in the Codes. Ample material would have been at the author's disposal had he really been dealing with primitive law (Post, Kohler). In referring to that and to primitive economics, government and culture, the reviewer's books could perhaps have been of some use.

It is impossible for me to go into the details of the history of such institutions as marriage, inheritance, property, contracts and courts, and to discuss my somewhat divergent view. The book, however, will cleanse the atmosphere, so that theories on primitive and ancient law more reliable than those challenged may be formulated.

RICHARD THURNWALD

Yale University

Civilisation and the Growth of Law. A study of the relations between men's ideas about the universe and the institutions of Law and Government. By WILLIAM A. ROBSON, Ph.D., LL.M., B.Sc. (Econ.); Barrister-at-Law of Lincoln's Inn; Reader in Administrative Law in the University of London. New York: The Macmillan Company, 1935. Pp. xv+354. $2.50.

"The object of this book is to depict the interactions between people's ideas about the universe on the one hand and the laws and government of mankind on the other." The author "endeavors to show how legal and political institutions have been influenced by magic, superstition, religion, and science, and how these great forces have in turn been influenced by the law." He calls his book "essentially an experimental study in the borderland between law, sociology, political science and certain other sciences."

Robson first considers the concepts of law set forth by a number of authors, and then proceeds to survey early forms of law and ruling in various primitive and ancient communities. He demonstrates that the laws given were in harmony with those powers which were supposed to govern the universe. Magic and superstition, ideas about religion, about God or the gods shaped the ideas of authority, exercised their effect on the substance and the working of laws, and, indeed, on the entire mechanism of government (p. 187).

The second part of the book considers "the influence which has been exerted by human laws and political institutions on men's conceptions of the universe and the general order of nature." Every society of human beings has a "framework of ideas" concerning the general constitution of the universe. The function of this framework enables men and women "to interpret in some measure their place in the general scheme of things, to reconcile their own lives with the phenomena which seem to lie outside those lives, to make both the individual and the group feel at home in the world." (p. 188). This is illuminated by a pronunciation of Demosthenes, a pupil of Plato, who declared: "This is law, to which it is proper that all men should conform, chiefly because every rule of law (*nomos*) is a discovery and gift of the gods. . . ." Demosthenes identifies the divine will, human reason, and the law of the State. The passage quoted was prefixed to the Digest of Justinian in the 6th century A. D. (p. 266).

In part III Robson proceeds to analyze the relationship existing between contemporary science and contemporary law. "The word law, in its scientific sense, has come to mean a theoretical principle deduced from particular facts, applicable to a defined group or class of phenomena, and expressed by the saying that a particular phenomenon always occurs if certain conditions be present" (p. 278). On the other hand, the law courts developed "the impartial methods of thought," which, like the man of science, strive "for self-elimination" to arrive at "opinions unbiased by personal feeling and supported by arguments as true for other persons as for himself." This is "recognized by all who have devoted themselves to the pursuit of scientific truth" (p. 287–88). The author holds that law is "not a chance product but the result of a reasonable impulse innate in human beings, a sociological

process arising from the material and spiritual needs of mankind" (p. 291). The word "law" denotes, both in science and in jurisprudence, order. "Scientific law reveals the existence of order where previously there was thought to be only fortuitous occurrence or supernatural intervention. Jural law ensures order where otherwise there might exist only chaos" (p. 292). Mere force in possession is not law; law involves a settled state of affairs, a common consciousness of consent, an orderly unity (p. 297–98). Science has borrowed the term law, as it has other terms such as inheritance and heredity, from the legal field. The scientists' concept of law has changed greatly in recent times, since they have become aware of the enormous complexity of natural phenomena. "The nature we study does not consist so much of something we perceive as of our perceptions; it is not the object of the subject of the subject-object relation, but the relation itself," says Sir James Jeans (p. 331). Ultimately, the origin of both jural laws and the laws of nature are to be traced to the human mind. But while in the juridical sphere the element of will is a dominant factor, in nature we find a mere succession of events.

The author refrains from investigating the problem of "free will." I think it could successfully be studied with reference to the structure of personality.

The book is that of a man not only of much learning but also of broad views and deep understanding.

RICHARD THURNWALD

Yale University

Religion and Science. By BERTRAND RUSSELL. New York: Henry Holt and Company (Home University Library), 1935. Pp. 256. $1.00.

Since this book is by Bertrand Russell it is naturally of high quality. Nevertheless there are a number of respects in which it is distinctly disappointing. For one thing, despite its title, the book is not a discussion of the broad theme of religion and science, one of the most important questions of the present time, but of the much narrower and much less important theme of Christian theology and science. It is Andrew D. White's famous "Warfare" redone and brought up to date. This is unfortunate. For as Mr. Russell himself says: "The man who feels deeply the problems of human destiny, the desire to diminish the sufferings of mankind, and the hope that the future will realize the best possibilities of our species, is nowadays often said to have a religious outlook, however little he may accept of traditional Christianity." And he adds: "In so far as religion consists in a way of feeling, rather than in a set of beliefs, science cannot touch it." This may be true if the feelings are pure feelings, not the feelings he has just described as religious. It seems all too obvious that science has, and will, touch religious feelings, if they have concrete aims and purposes. It is in this connection precisely that the problem of science in relation to religion has become acute in the contemporary world.

For another thing, Mr. Russell insists repeatedly that knowledge is at-

tainable solely by means of scientific methods. How broad is his definition? Does it include the methods of the social sciences? The implications of his whole treatment are that true science is limited to physics and chemistry. This again sidetracks one of the most pressing problems of the present time. How is intelligence to be brought to bear upon the general social and political situation? How are we to solve problems with the greatest possible objectivity which cannot as yet at least be reduced to physical-chemical terms? In a very interesting, though not novel chapter, "Science and Ethics," Mr. Russell remarks: "The sort of life that most of us admire is one which is guided by large impersonal desires; now such desires can, no doubt, be encouraged by example, education, and knowledge. . . ." Here he touches upon the crux of the matter. This is where light is needed. In what ways can scientific method help in its development of these personal desires for a wider-than-personal good, and how can such desires, through coöperation with science, arm themselves with the intellectual tools, the most practically effective methods of attaining their ends?

Once more, although there are outcropping remarks, witty, biting, well-aimed, which show a strong preference for a better type of society than the existing one, the discussion as a whole seems more like one side of an academic debate, presented by a master of the art, than like an active sympathetic participation with men and women in the solution of harassing problems growing out of the conflict between science and religion broadly conceived. Arguments usually made for the soul, free will, mystical truth, cosmic purpose are riddled by the author's dispassionate but deadly logic. Were I a member of a jury I would unhesitatingly vote for Mr. Russell's side. And then I should go out into the night feeling that the points really at issue, the aims and interests embodied, however erroneously, in these exploded conceptions, had been completely ignored, and the problems which they represent in the struggles of men had been treated as if they were of no consequence.

M. C. Otto

University of Wisconsin

The Sociology of Invention. By S. C. Gilfillan. Chicago: The Follet Publishing Co., 1935. Pp. xiii. +185. $2.00.

Much of the material in this volume will already be familiar to many. The essays collected in the present study have appeared in the Journal of the Patent Office Society. Furthermore, the doctrine presented is a development of the views familiar to us in the papers of Ogburn and Dorothy Thomas. There is much new material, and the element of process in technological development is treated much more adequately, but the basic concepts are very similar.

The present volume is described as an "essay in the social causes of technic invention and some of its social results." This limited objective is clearly recognized by the author in the general plan of the book, but the thirty-eight principles of invention and many portions of the text present

bold and confident generalizations which involve many problems pertaining to the general theory of invention and social evolution. Despite these broad implications, little positive attention has been given to the psychology of invention or to the historical aspects of social evolution. The author makes skillful use of the material on the history of the ship, separately published in a companion volume, but the general problems really require a somewhat broader basis than the highly specialized case study.

Dr. Gilfillan makes no formal distinction between the single invention and the series of inventions which are built into an achievement of practical importance. The term "invention" is commonly used in the text with reference to the organized series of inventions, so that there is not even a specific term to be applied to the actual unit of innovation. For actual historical study such a distinction is indispensable.

If attention is to be given to the individual act of innovation, some account must also be given of the process by which these inventions and discoveries are made. This problem receives no specific treatment, though it is involved in any discussion of parallel invention. The abandonment of the concept of invention as a pure transcendental process makes it especially important to analyze precisely the mental processes of the inventor. The fact that invention is a conditioned activity of the mind does not necessarily lead to the extreme form of determinism implicit in the doctrine that "invention is inevitable." Some would perhaps reach such a conclusion even after careful psychological analysis, but other positions are tenable.

Dr. Gilfillan presents an interesting discussion of the life cycle of "inventions" without explicit recognition of the relation of this concept to multilinear concepts of social evolution. His whole discussion leads strongly in this general direction, but some phenomena could be treated more decisively if related to this phase of the theory of social evolution. The study is significant and helpful, but it is somewhat less conclusive than one might presume from the sharp formulation of the thirty-eight principles.

ABBOTT PAYSON USHER

Harvard University

Personality Maladjustments and Mental Hygiene. A Textbook for Psychologists, Educators, Counselors, and Mental-Hygiene Workers. By J. E. WALLACE WALLIN. New York: McGraw-Hill Book Company, Inc., 1935. Pp. xii+511. $3.00.

Dr. Wallin, who is Director of the Division of Special Education and Mental Hygiene for the Delaware State Department of Public Instruction and the Wilmington Public Schools, here presents the results of many years of clinical work and of collecting personal data from students about their "early adjustment difficulties." Roughly, a third of the book consists of this autobiographical case material from students. Part I deals with the concept and objects of mental hygiene, the relations of mental hygiene to interaction processes in primary groups and to educational processes and methods, and presents a practical classification of the types of *children*

with whom mental hygiene is concerned. Part II deals with specific symptom-groups and their treatment. These symptoms are placed in 15 categories, some of which are identical with much-used psychoanalytic concepts, and some of which are of a general or practical order, such as "trial-and-error adjustments" and "procrastination."

Dr. Wallin appears above all as the practical educator, seeking to spread the generally accepted thought-ways, attitudes, and techniques of modern mental hygiene, through teachers, parents, and other counselors, to the masses of children for whom prolonged individual treatment is either unnecessary or impossible. In this pedagogical task, the present book seems to the reviewer to promise high effectiveness, to be one of the best of its kind. Its wealth of concrete material is often rather vivid and maintains in the reader a high level of spontaneous interest. There are many practical suggestions, and a rather remarkable collection of poetical and literary quotations which help to drive home mental hygiene principles.

The book, however, is not designed to contribute to the advancement of mental hygiene as a *science* on either the individual or the sociological side. It is eclectic but not critically discriminating. There is a lack of logical clarity and order, a repetitiousness (which the author admits and justifies), an absence of adequate documentation, little or no recognition of quantitative or constellation-seeking research in personality maladjustments, or of the relation of cultural patterns or values as such to psychopathology.

Yet as a comprehensive though loose *inventory* of types of maladjustment and methods of treatment the book may serve scholars very well, as a source of illustrations for lectures or a check-list of ideas for research. Many of the case reports describe, from the patient's point of view, the event or treatment through which a given maladjustment was cured. Such data, if analyzed according to some logical system and then treated statistically, might give us a new and rather valuable research method. Some control measure would be needed to test the validity of "patient's own stories" of their cures.

JOSEPH K. FOLSOM

Vassar College

The Incidence of the Terror During the French Revolution. A Statistical Interpretation. By DONALD GREER. Cambridge: Harvard University Press, 1935. Pp. 196.

More than twice as many persons are killed by automobiles in the United States every year as were executed in France during the year and a half of the Reign of Terror. Yet Mr. Greer seems to recognize the futility of his laudable effort to destroy the old idea that France was a sea of blood during the Terror. His statistics are all down in unimpeachable array, but they will not destroy the stereotype. As in the case of the Russian Revolution, the fact that there were fewer persons executed than were killed in any one of several battles fought during the period has no effect on the popular mind. The peculiar horror attaching to the French and Russian Terrors

has little connection with the executions or with the numbers executed. It is far more basic. It is revulsion against the overthrow of property institutions. Both Terrors were marked by confiscation and expropriation of the property of the formerly ruling classes. Subconsciously or otherwise, the destruction of property rights is a far more terrible thing to most respectable people than destruction of human life. But the open acknowledgment of this fact is not moral, so our subconscious minds transfer the terror to an ethically appropriate object.

Our author does not make this point, but he does prove some other things of interest. The Terror was not class war. More day laborers were executed than dukes and marquises. The nobles put to death totalled only one-third of one per cent of the nobility. The same percentage holds for the clergy. The Terror was intra-class war. Its victims form a complete cross-section of the social world of the old regime. The majority of them deserved death if we admit the right of a government to execute those who take up arms to destroy it, or who plot its overthrow. The first French Republic put its internal foes to death irrespective of their social rank or economic status. In two excellent shaded maps, Mr. Greer shows that the number of executions in the different departments corresponds very exactly with the extent of the revolt in those departments.

Statistical tables make up a considerable part of the book. They are well arranged. The bibliography is good.

LYFORD P. EDWARDS

Bard College,
Columbia University

Head, Heart and Hands in Human Evolution. By R. R. MARETT. New York: Henry Holt and Co., 1935. Pp. 303.

In this collection of miscellaneous essays, Dr. Marett, Rector of Exeter College, Oxford, and dean of English anthropologists, merely serves his old wine, sometimes mellow, but often fermented from repeated exposure, in new glittering bottles. He is characteristically benign, humane, and humanistic in his treatment of native life, and applies an evolutionary approach divested of its unilinear postulates. His style is, as ever, genial and facile. The text is replete with felicitous formulations, yet at times so overburdened with literary allusions and cast so consciously by one enamoured by the art and artifice of words, that the development of the author's ideas is impeded and his scientific judgments blurred. The essays have slight continuity. Originally prepared for diverse audiences, they range from a limited academic to a widely popular appeal. Their arrangement, which gives the book its title, is arbitrary. The opening chapters on theory, comprising addresses to the Sociological Institute, are here offered, says the author, to clarify "how to use, and to keep, one's head in the matter"; then follows the discussion of the central topic, religion, "which in its pretheological phase, at any rate, is shown to be essentially an affair of the heart"; and finally two exceptionally compact essays on primitive technology.

In one of the earlier essays, the author declares that "Sociology makes no affirmations about values, but so presents facts that they can be used for a criticism of values." But value-judgments pepper this volume, especially in the chapters on "pre-theological religion." "The best way of looking after a body is to cultivate a soul," he writes, and thus he interprets the religious feeling, thinking and action of primitive men as commendable functional devices of adjustment to a world pregnant with hazards. He regards magic as "religion in the making," and insists that Frazer's differentiation between these two forms of manipulating the supernatural is unjustifiable. His overt religious bias leads him to hail the "progressive triumph of spirit over matter," to claim that religion is "the mother both of the sciences and of the liberal arts," and to maintain that "materialism acts as a drag on moral progress."

Marett displays pronounced insularity in his complete neglect of the contributions of American ethnologists, some of which are particularly relevant to his discussion of primitive religion. Knowledge of Goldenweiser's critique of totemism, for example, might have prevented his chapter on this subject from being so inconsequential.

BERNHARD J. STERN

Columbia University

Primitivism and Related Ideas in Antiquity. By ARTHUR O. LOVEJOY and GEORGE BOAS. Baltimore: The Johns Hopkins Press, 1935. Pp. xv+482. $5.00.

This constitutes the first volume of an analytical and documentary record of man's appraisal of the historic process in general and of its dominant tendencies, particularly in respect to the direction of social development. In addition to this major history, a number of collateral monographs pertaining to special aspects are being published. In this initial volume is included a wealth of sources (both original texts and translations) relating to the different varieties of primitivism, with particular emphasis upon the relevant works of Plato, Aristotle, Lucretius and Cicero. No pretense is made of introducing directly sociological analysis, but virtually all of the material has important sociological implications.

This material is classified according to a number of "unit-ideas." The main distinction is that between chronological and cultural primitivism. The first refers to those theories which maintain that the concentration of some value (happiness, morality, etc.) was greatest at some primeval period in man's history. Manifestly, then, this doctrine is antithetic to a law of progress. "Cultural primitivism is the discontent of the civilized with civilization, or with some conspicuous and characteristic feature of it." This view leads to the projection of a culturally simple condition as an ideal. Both of these basic types of primitivism are broken down into constituent elements.

Primitivism itself is part of a larger system of ideas which uses the term "nature" to express the norm for human values. The word nature, however,

has been accorded many, often contradictory meanings. The authors have painstakingly distinguished sixty-six senses of the conception! A careful analysis of the varying concrete meanings given this abstract concept would clearly afford a considerable contribution to *Wissenssoziologie*, for as Mannheim, Pareto and others have indicated, such shifts in meaning attached to the same term are symptomatic of the nature of socio-cultural influences upon thought.

It is significant that anti-primitivism—which, though it can by no means be equated with progressivist ideas, is yet a necessary element of them—was fully manifest in the late fifth and early fourth centuries in Athens. For anti-primitivism, then as now, is closely linked with the acceptance of technologic criteria of progress, and is an integral part of any culture which has actually witnessed great scientific and technologic advance. Thus, the contents of this book, cogently analyzed, may be coupled with René Hubert's phenomenology of progress to form a basis for further theoretical developments in this field.

As the authors indicate, the student of ideologies will find suggestive parallels between ancient and modern thought regarding primitivism. Classical primitivism manifestly bears a significant similarity to the somewhat more elaborated ideas found in Freud's *Civilization and Its Discontents* and Spengler's *Man and Technics*.

The material contained in this volume is thus of considerable importance for the sociologist, leading the way, as it does, for the inductive study of ideas and society. The various ideologies therein presented and classified can be subjected to various analyses: Which of these doctrines were predominant at different periods in Greek and Roman culture? What systems of value and social structure do they presuppose and reflect? What social functions do they perform? In short, the culturally diagnostic value of these ideologies still remains to be determined.

It has been the perennial lament of the historical sociologist that historiographers have not assembled those data which are of most value for sociological purposes. This criticism would be sadly misplaced in this instance, for Professors Lovejoy and Boas have organized data which simply demand the attention of the sociologist. In this respect, the work does much more than fulfil its avowed function. One eagerly awaits the further volumes of this history.

The publishers are to be congratulated upon the handsome format of the volume.

ROBERT K. MERTON

Harvard University

An Introductory Sociology, By KIMBALL YOUNG, 1934. Pp. 615. *Source Book for Sociology*, 1935. Pp. 639. New York: American Book Co. Each $3.50.

Professor Young can hardly claim to have solved satisfactorily the problem of the logical arrangement of the material of the introductory course.

He merely adds one more variant to the numerous ones on the market. But perhaps any ordering of personality and process, culture and control will do, if a courageous attempt is made to string the discussion of these concepts on a thread of meaning.

The first section of the book discusses groups, culture and personality. This is followed by geography, race and population. Institutions comprise part three, social processes part four, and a section on social control concludes the text. The contributions to our understanding of man's social life made by cultural anthropology and social psychology are brought together with those of sociology proper. Young has combined these topics and approaches in a workman-like, if not an exciting, fashion; the result is a textbook refreshingly free from bias and naïveté. If for no other merit, it would deserve the extensive adoptions it is receiving for being the first book adequately to draw upon the vast resources of the Encyclopedia of the Social Sciences.

The Source Book designed to accompany and supplement the Introductory Sociology is remarkable not only for the enormous amount of material it includes, but for the care with which the selections have been made and the recent date of much of the writing. There is some loss of interest due to the fact that the readings are drawn rather from learned than from popular sources, but this procedure has obvious justification. This reviewer would prefer more emphasis on social change, but why be picayune? Both volumes can be warmly recommended to all save those who insist upon a biological or a community approach.

W. Rex Crawford

University of Pennsylvania

Sociology. By Morris Ginsberg. New York: Henry Holt and Co. (Home University Library), 1934. Pp. 225. $1.00.

This little volume opens with a chapter on the "Scope and Method of Sociology." Assuming that in the most general sense "sociology is the study of human interactions and interrelations, their conditions and consequences" with the whole life of man in society as its field, the author immediately turns to the problem of delimiting the field to manageable proportions. The author reviews two resulting conceptions of sociology: the effort of Simmel and his followers to confine sociological study to certain defined aspects of human relationships, and the other effort to make of sociology a general social science which would "bring the results of special disciplines into relation with each other, to deal with the general conditions of social life, which, because of their very generality, are often ignored by specialists, in short, to view social life as a whole."

The author states his own conception of sociology, or of the functions of sociology, under three heads: (1) It seeks to provide a "classification of types and forms of social relationships, especially of those which come to be defined in institutions and associations"; (2) it seeks to determine the

relation existing among the different factors of social life; (3) "it endeavors to disentangle the fundamental conditions of social change and persistence." The methods of the discipline follow from this conception of its scope. The chapter closes with a section devoted to a discussion of the relation of sociology to social philosophy.

The following chapter on "Society, Culture and Civilization" discusses further, chiefly by illustration of important problems, the general scope and methods of sociology. Chapter three is given to a consideration of the influence of the physical environment and of race; chapter four is given to a psychological analysis of the elements of animal nature "which bear most directly upon the relations of man and man." The author summarizes his intent in the remaining chapters of the book as follows:

> The discussion which follows, seeks to give some account of the various forms of social relations which have been outlined. . . . No detailed description could, of course, be attempted. Chapter V deals with the general principles of social organization, as reflected in the growth of political communities, and with the various types of social control. Chapter VI is concerned with class structure and economic organization. Chapter VII discusses in very brief outline the principal trends of mental development, in the fields of morals, religion and science. The concluding section raises the problem of the relations between these different spheres of social life, and indicates the future tasks of sociology.

E. B. Reuter

University of Iowa

Introductory Sociology. By Raymond W. Murray. New York: F. S. Crofts and Co., 1935. Pp. xvii+423.

This book is announced as "a standard college text which, while clinging to basic Catholic ethical teachings, will follow the scheme of the introductory course as it is usually given in most American colleges." It devotes 99 pages to genetics, race and population; 83 pages to personality and social adjustment; 110 pages to primitive man; and 55 pages to poverty, crime and family disorganization. A considerable fraction of the space is taken up with exposition of and argument for distinctively Catholic points of view.

The usual Catholic positions are taken relative to birth control, sterilization and divorce. Psychiatrists are reproached with having, in some instances, attempted to usurp the functions of the Catholic confessor, while applying the philosophy of paganism. Catholics "must believe, as a matter of God's revelation, that all of us are descended from a single pair and so contract the stain of original sin acquired by the disobedience of Adam and Eve." This doctrinal necessity is reconciled with prehistory by the hypothesis of placing Adam, Eve, Cain, Abel, etc., in the Tertiary Epoch, regarding Pithecanthropus and Neanderthal as their descendants, and supposing that the deluge may possibly have occurred in Quaternary Time and been confined to Mesopotamia. Adam and Eve received a direct revelation of religion from God. "Since all modern primitives have descended from our monotheistic First Parents, . . . all other forms of religion must be of later origin."

"Sociology has trifled with religion from the start. . . . Much of our sociology is cynical and lacks the *will* to believe. . . . Most American sociologists . . . often advocate divorce, contraception, sterilization, determinism, custom-made morality, and materialistic evolution, among other things contrary to Catholic, if not also to general Christian morality."

This book will be valuable to teachers of sociology in Catholic institutions, and also to non-Catholics who wish to gain insight into the Catholic culture-complex.

HORNELL HART

Hartford Theological Seminary

Ideas in Motion. By DIXON RYAN FOX. New York: D. Appleton-Century Company, 1935. Pp. vi+126. $1.25.

Everyman His Own Historian. By CARL BECKER. New York: F. S. Crofts and Co., 1935. Pp. viii+325. $2.50.

These collections of essays by two distinguished American historians are eloquent of the sociological trend of interest on the part of practitioners of the older discipline. Fox's "transit" of civilization turns out to mean exactly what sociologists mean by "diffusion," and his opening essay outlines a valuable theory of the process we call "acculturation." Later, seeking a "synthetic principle for American social history," he prefers "social evolution" (which he equates with "differentiation") to conflicts of group interest. Moreover, he emerges with at least two "laws," supported by considerable empirical data. "Professional competence rises through provincial to metropolitan status by the process of reception, attendance, dependent organization, and self-maintenance." "The cast-off garments of the intellectuals of one age are found, albeit soiled and ragged, on the backs of the ignorant many of the next."

While Fox pursues the rise of professional institutions, Becker's concern is with personality. The essay on Madame Roland is worth many case histories as an analysis of roles, compensation and identification, public and private attitudes. But his wider interest is in the social conditioning of ideas—indeed, in the interaction of ideas in the behavior of persons in what he has elsewhere termed "climates of opinion." It would be easy to tag him with hard words like "historism" or *Wissenssoziologie.* But he disdains labels. Like Turner his master, he would dislike to be called either an historical sociologist or a sociological historian. In fact he is not to be trapped with any pinch of academic salt. Is he objective? Heaven forbid! Becker will show you what nonsense that is. Ah, then, he is a social philosopher, expounding his incessant liberty and equality and democracy as absolutes! But that is what he does not do. Aware of what are social values for him, he will not identify them with the cosmic process. They are perhaps but rationalizations that play their passing role in the social scene. Their quality is that they are his, perhaps a lag from a more congenial phase of

society. None the less he likes them, and he likes to study the sorts of things he likes to study, and he will go on questioning his thoughts about the ideas people think they think until he is suppressed.

It is idle to ask on the basis of these volumes what distinguishes social history from sociology, or from the things sociologists write about. But certain striking qualities leap to the eye. These books are readable. Artistry and a lively prose style have gone into their composition. The mechanics of research are carefully concealed and the concepts employed are those which have been passing from sociology into general currency. Is it possible that much of importance about society can be said in the vocabulary of educated laymen, without resort to coefficients of correlation?

The question recalls another quality of these essays. Both writers face clearly and in good humor questions of methodology, which stir professional historians no less vitally than they do professional sociologists. Is there some special grace accruing from the study of history that enables its practitioners to disagree so much more graciously—in public at any rate—than has seemed possible for rival champions in the field of sociological method? If so, what is one to say of Fox's witticism about his own sociological law—"When baldly stated it is so ponderously cryptic as to be unintelligible, and when explained it is so obvious that it need not have been stated at all"? Could one say that in looking for "laws" he is illustrating his own theorem as to the downward trend of ideas?

LELAND HAMILTON JENKS

Wellesley College

Girls on City Streets: A Study of 1400 Cases of Rape. By JACOB A. GOLDBERG and ROSAMOND W. GOLDBERG. New York: American Social Hygiene Association, 1935. Pp. xii+376.

Most of this book consists in summarized cases of girls aged four to sixteen who were seduced or raped by acquaintances, strangers, boarders, or relatives. Several chapters, all stressing some particular factor, present twenty cases each. One chapter deals with runaway girls. Other sections deal respectively with girls in broken homes, crowded homes, poor environment, and still others with very small children, with incest cases, and with girls of normal and subnormal intelligence. The last part of the book provides figures regarding the age distribution, age at first sex experience, and school status of the girls, together with statistics on the kinds of places where violations occurred and the types of men responsible. Final discussion concerns the influence upon children of sex movies, burlesque theatres, closed dance halls, and racy magazines.

Many of the case histories are startling but too brief to give the reader any insight into causes. The authors' interpretation is platitudinous and moralistic.

KINGSLEY DAVIS

Smith College

Triunfalmente (*Acción Batllista*). By Rómulo Nano Lottero. Montevideo: José Maria Serrano, Editor, 1933. Pp. 101. *La Revolución del Machete: Panorama Político del Uruguay*. By Emilio Frugoni. Buenos Aires: Editorial Claridad, 1934. Pp. 252. 80 ctvos. *Con Sandino en Nicaragua*. By Ramón de Belausteguigoitia. Madrid: Espasa-Calpe, S.A. 1934. Pp. 245. 5 ptas. *Nuestra Evolución Político-Social* (1900–1930). By Jorge Gustavo Silva. Santiago de Chile: Imprenta Nascimento, 1931. Pp. 166. *Construyendo el Aprismo*. By Haya de la Torre. Buenos Aires: Editorial Claridad, n.d. Pp. 240. 60 ctvos. *La Revolución del 6 de Septiembre*. By Carlos Cossio. Buenos Aires: Juan Roldán y Cia., 1933. Pp. 212. *Polícia Intelectual*. By Ramón Doll. Buenos Aires: Editorial Tor, 1933. Pp. 171. *Votar no Es Elegir*. By Roberto Kurtz. Buenos Aires: Luis Veggia, Editor, 1931. Pp. 112. 1 peso. *El Tramonto del Parlamentarismo*. By Fidel A. Zárate. Lima: Imprenta Minerva, 1933. Pp. 263. $1.90. *Libertad y Autoridad*. By Alfredo Fragueiro. Cordoba (Argentina): Imprenta de la Universidad, 1933. Pp. 281. *Democracia: Mal Menor*. By Ramón Doll. Buenos Aires: Imprenta Araujo Hnos., 1934. Pp. 47. *Nacionalismo y Socialismo*. By Adolfo Dickmann. Buenos Aires: Author, 1933. Pp. 144. *El Imperio Soviético*. By Dionisio R. Napal. Buenos Aires: Editorial Stella Maris, 8th Ed., 1933. Pp. 315. *Engaños y Errores del Comunismo*. By J. Conangla y Fontanilles. La Habana: Imprenta "La Milagrosa," 1934. Pp. 277. $1.00. *Government and Politics of Italy*. By Henry Russell Spencer. New York: World Book Co., 1932. $1.60. *Se Acerca la Guerra*. By V. Lillo Catalan. Buenos Aires: A. García y Cia., 1933. Pp. 31. 40 ctvos. *Hay Novedad en el Frente*. By Helen Zenna Smith. Barcelona: Casa Editorial Maucci, n.d. Pp. 249. *La Asonada*. By José Mancisidor. Editorial Integrales, Jalapa, Mex., 1931. Pp. 199. 2 pesos. *Vers la Paix*. By Alberto Torres. Rio de Janeiro: Graphica Ypiranga. Pp. 152. *El Perfil Americano*. By Arturo Mejía Nieto. Buenos Aires: Liberias Anaconda, 1933. Pp. 175. *Fuerzas de Unificación*. By Octavio Méndez Pereira. Paris: Editorial "Le Livre Libre," 1934. Pp. 176. *Atenea Política*. By Alfonso Reyes. Santiago de Chile: Ediciones Pax, 1933. Pp. 59. 75 ctvos. *Los Problemas de la Unificación Americana*. By Eugenio Orrego Vicuña. Santiago de Chile: La Universidad, 1933. Pp. 50.

In these 23 volumes dealing with Revolution and Authority, mostly in Latin America, we find a wide range of emphasis, all within the same gen-

eral theme. Together they present a pretty consistent picture of what the political sociologists are thinl .ig along this line. They group easily under five headings.

1. The first volume documents the successful formation of the Batllista party in Uruguay, beginning in 1919 (José Batlle y Ardoñez is a descendant of the Battles of Chapel Hill, N.C.). The second volume tells the story of the Socialist-Communist revolution against the domination of the Batllistas and other conservative agrarian and capitalistic parties in 1932–1933. Together these volumes of Nano Lottero and Frugoni present in the words of the leaders of the two movements an excellent documentary case study in the dynamics of Latin American politico-social tactics in their more peaceable revolutionary form. The story of Sandino's revolution in Nicaragua is a case record of a particular kind of revolutionary tactics—the Fabian methods of the "bushwhacker" who retreats into the forest and the mountains, from which he can be dislodged with great difficulty, with roads and communications and native sympathies what they are. The interest of the volume is increased by a number of photographs and the peculiarly idealistic personality of Sandino.

Silva's work on the recent politico-social evolution of Chile describes both violent political revolution and the more peaceful parliamentary social revolution which have been taking place in that country. Written by a professor in the University of Chile, it contains a fundamental sociological analysis of its theme. *Construing Aprism* is a collection of the documents relative to this radical party in Peru which contributed much to the revolution overthrowing Leguia in 1930. Haya de la Torre is a sort of Garibaldi of Peru. Many years in exile, he spent his youth studying at the leading European universities and in stimulating the Apristic revolutionary movement at home. These documents of the movement from his pen were published on the 400th day of his imprisonment after his return to Peru.

2. The second group of works do not describe revolutions, but discuss the problems of political and social reconstruction after revolutions have occurred. Cossio's book is a penetrating historic and philosophic analysis of the conditions back of the Argentine revolution of 1930 which overthrew Irigoyen and established a temporary Fascistic dictatorship. Since 1912, Argentina has had the secret ballot, but it still needs a responsible press which will not mislead the people on political and social issues, a civil service system which will prevent public employes from being intimidated into a political gangster organization, and an effective control over the legislative activities of the people's representatives. This is a profoundly valuable monograph in political sociology. Doll, of the staff of *Nosotros*, criticises trenchantly the corrupt liberalism which precipitated this revolution, the short-sighted and lone-handed tactics of communism, and the insincerity and deceptive policies of Fascism as substitutes for the old political order in Argentina. While Doll represents basically the socialist viewpoint, he appeals to all types of opinion to rally against Fascism. Kurtz finds the source of the Argentine revolution in the current political philosophy which teaches the voters to dogmatize parties and party leaders

and to hate opposing parties; and especially in the crooked political manipulation that goes on behind the political scenes. His remedy lies in the imitation of the political methods of the United States, where issues are made to replace partisan rancor and the primary election, referendum, recall, etc. render fraud more difficult and the triumph of an enlightened popular will more probable. Kurtz shows a familiarity with our political literature that is unusual in Latin America.

Zárate's *The Passing of Parliamentarianism* maintains that this system was never anything but a device by means of which the capitalist class came into power in opposition to the noble and clerical classes and has held its power against the proletariat. It is a singularly erudite work, showing the author's close familiarity with the whole history of political and social institutions and their theorists. His substitute is a dictatorship of the proletariat based on social science and protected from abuse of power by popular checks and controls. Professor Fragueiro, in *Liberty and Authority*, attempts to explain the dictator and the remedy for him. When, for any reason, the old legal system of order breaks down—usually because the general struggle for special privileges culminates in license—the dictator is the natural and necessary recourse to restore order. But the order of the dictator is partisan, selfish, corrupt: he establishes a new legal system of sanctions in his own favor. The remedy for this is that the people make themselves conscious of their fundamental social problems and relationships and reëstablish a new and democratic legal system of sanctions which will protect their rights. In order to do this they must dominate the system of social controls, and especially the press. Doll's *Democracy, The Lesser Evil* points out the failures of democracy, suggests methods of remedying them, and contends that the Fascism so much feared in Argentina (one-third of the population is of Italian descent) is a much greater danger. Congressman Dickmann's efforts are bent, in *Nationalism and Socialism*, on showing that socialism is not opposed to the traditional nationalism of Argentina, but is an outgrowth of the original Independence movement; also that foreigners (who make up a considerable portion of the personnel of Argentine socialism) have always been strong supporters of national liberties and patriotism.

3. The Vicar General of the Argentine Navy (Napal) has written a very popular work (more than 100,000 copies had been sold in 1933) criticizing Soviet Russia. It is well organized and very clearly written. It traces the Marxist doctrines down to the time Lenin embodied them in the Soviet regime. On top of this sketch he builds a lurid picture of class privilege, terror, exploitation, immoral and anti-religious behavior, propaganda, and cultural decline, the whole crowned with a threat to the civilization of the whole world. While the reviewer cannot accept this picture as photographic, it is nevertheless powerful and persistent. The author presents only one side of the picture, and that not always clearly supported. Conangla's treatment of the same theme is more sober, but scarcely less one-sided. The author devotes much of his space to showing that communism would not

be feasible in Cuba. The fact is that a pro-communistic movement has grown rapidly in Latin America, due to the political exploitation so rampant there, and those who hold to the capitalistic order fear it greatly and are making strong propaganda against it. Spencer's work describes the actual and paper governments of Fascist Italy as they are today.

One of the survivors of the Great War—Lillo Catalan, in *War Is Coming*—gives us a criticism of the Fascistic insincerities of Keyserling and of the old war lust resuscitated in modern Nazi Germany. Two novels—those by Smith and Mancisidor—paint realistic pictures of war in action, one on the canvas of the Great War and the other on the smaller scene of the Mexican Revolution. Mancisidor has put into his novel the fire and romance that are characteristically Mexican, while at the same time he makes his story preach powerfully that war is assassination. A Brazilian statesman—Alberto Torres—turns our attention toward plans for peace and a scheme for the organization of courts of international justice. Although a strong nationalist, he realizes and proclaims that nations are no longer safe without international organization. He wrote before the day of the Italian invasion of Abyssinia.

While waiting for the larger world order to assert itself, the Latin Americans have been endeavoring to form some sort of coöperative unity of the nations lying below the Rio Grande. They are not satisfied with the Pan-American union and wish to constitute a family of nations of their own. But not all of their writers are unfriendly to the United States. Mejía Nieto's *American Profile* attempts to be objective about the matter and in a series of clever dialogues has two students argue the question out as to the motives, methods and competence of the policies of the United States as against those of Latin America. This is indeed interesting and instructive reading, for it touches upon almost every question at issue. Méndez Pereira, a Panaman diplomat, comes out clearly for the United States and her policies. He says she has a right to demand that the Latin-American countries be responsible for order and for their pledges within their borders, if they claim the right to be independent nations. He discusses a wide range of questions relative to the two continents. Among other things, he disagrees with most Latin Americans and regards the people of the United States as preëminently idealists rather than as dollar chasers—the common conception held by our friends to the south who are so expert at getting in between us and the dollar we are supposed to chase. Alfonso Reyes, the distinguished Mexican professor of law, and diplomat, states a concrete plan of unification in his *Political Athenaesism*. It is the establishment of exchange professorships and studentships among the various Latin-American countries. Orrego Vicuña traces the whole history of the movement toward unification throughout the eighteenth and nineteenth centuries and proposes a Society of American Nations. His brochure, although small, is filled with information and analyses in the highest degree interesting.

L. L. BERNARD

Washington University

Indian Life of Long Ago. By Reginald Pelham Bolton. New York: Joseph Graham (Schoen Press), 1934. Pp. xiv+168. $4.00.

This interesting and well-written volume affords an authoritative, as well as interesting, study of the Indian aborigines who once inhabited the area of present day metropolitan New York. The Lenape, commonly called the Delawares, possessed a large number of settlements in Manhattan, the Bronx and Westchester County, Queens, Richmond and Long Island, as well as numerous sites on the New Jersey side of the Hudson River. The traditions and legends of these people indicate that long before the coming of the white man they themselves were newcomers to the present metropolitan region, probably having migrated from territory around the Great Lakes. Here, in their new home with the bounties of nature at hand providing an abundance of sea food, game, and opportunities for agriculture, the Indians prospered to such an extent that their density of population was perhaps greater than anywhere else in the country.

The Lenape are depicted as fairly well advanced in agriculture, having developed by careful selection several varieties of maize and of beans; they were good fisherman, and possessed skill in pottery making. Their outstanding skill, however, was in the manufacture of wampum beads, so that New York was even then the money center of America. This industry furnished an excellent example of local specialization of labor, largely owing to the abundance of clam and conch shells found on the shores of Long Island, known to the Indians as "Meht-anaw-ack," or "Ear-shell-country." A number of maps indicate the various known settlements of Indians in the New York area, and it is striking to observe that practically all of them were water-front communities.

This work is notable for its clear and concise treatment of Lenape life and customs on the site of the great metropolis, as reconstructed from historical records, artifacts and other existing data accumulated by the author in the course of some thirty years of research. It should prove a worthwhile aid to the student of primitive culture and social origins.

EARL E. MUNTZ

New York University

BOOKS RECEIVED

War and Wages. By Robert E. Adams. New York: Primrose Publishing Corporation, 1935. xiii+324 pp. $3.00.

An Introduction to Contemporary German Philosophy. By Werner Brock. London: Cambridge University Press, 1935. xx+144 pp. $2.00.

Capitalism and Its Culture. By Jerome Davis. New York: Farrar and Rinehart, 1935. xx+556 pp. $2.25 (college edition).

Length of Life: a Study of the Life Table. By Louis I. Dublin and Alfred J. Lotka. New York: The Ronald Press Company, 1936. xxii+400 pp. $5.00.

An Introduction to the Study of Society: An Outline of Primary Factors and Fundamental Institutions. By Frank Hamilton Hankins. Revised edition. New York:The Macmillan Company, 1935. xxii+808 pp. $4.00.

Economics, Sociology and the Modern World: Essays in Honor of T. N. Carver. Edited by Norman E. Himes. Cambridge: Harvard University Press, 1935. xxii+330 pp. $5.00.

Hawaii, a Pageant of the Soil. By Jean Hobbs. Stanford University, California: Stanford University Press, 1935. xviii+196 pp. $2.50.

Heredity and the Ascent of Man. By C. C. Hurst. New York: The Macmillan Company, 1935. x+138 pp. $1.50.

Marx and the Trade Unions. By A. Lozovsky. New York: International Publishers. 188 pp. $1.75.

The Individual Criminal: Studies in the Psychogenetics of Crime. By Ben Karpman. Washington: Nervous and Mental Disease Publishing Co., 1935. x+318 pp. and 2 tables.

A Study of Rural Society, Its Organization and Changes. By J. H. Kolb and Edmund deS. Brunner. Boston: Houghton Mifflin Company, 1935. xiv+642 pp. $3.50.

A Handbook of Social Psychology. Edited by Carl Murchison. Worcester, Mass.: Clark University Press, 1935. xii+1196 pp. $6.00.

Farewell to Poverty. By Maurice Parmelee. New York: John Wiley and Sons, Inc., 1935. xvi+490 pp. $2.50.

Never Say Die, an Autobiography. By John Paton. New York: Longmans Green and Co., 1936. xiv+336 pp. $2.50.

The Political Clubs of New York City. By Roy V. Peel. New York: G. P. Putman's Sons, 1935. xii+360 pp. $3.00.

Leadership or Domination. By Paul Pigors, with an introduction by Richard C. Cabot. Boston: Houghton Mifflin Company, 1935. xiv+354 pp. $3.00.

Law and the Lawyers. By Edward S. Robinson. New York: The Macmillan Company, 1935. xiv+348 pp. $2.50.

The Growth of Population in Louisiana, 1890 to 1930. By T. Lynn Smith. Louisiana Bulletin No. 264: Louisiana State University and Agricultural and Mechanical College Agricultural Experiment Station.

Contemporary Problems in the United States. By Horace Taylor with the collaboration of Columbia College Associates in Economics, Government and Public Law, History, and Philosophy. New York: Harcourt Brace and Company, 1935. 2 volumes, x+508 and viii+532 pp.

Readings in Recent Political Philosophy. Selected, abridged and edited by Margaret Spahr. New York: The Macmillan Company, 1935. xvi+776 pp. $3.90.

American Family Laws: Volume Three, Husband and Wife. By Chester G. Vernier. Stanford University, California: Stanford University Press, xxx+684 pp.

A Footnote to Folly. By Mary Heaton Vorse. New York: Farrar and Rinehart, 1935. viii+408 pp. $3.00.

Periodical Literature

BRITISH JOURNALS

KINGSLEY DAVIS
Smith College

Annals of Eugenics (Vol. 4, Part 3, December, 1935).—Torsten Sjögren: Investigations of the Heredity of Psychoses and Mental Deficiency in Two North Swedish Parishes, pp. 253–318.

The Cambridge Historical Journal (Vol. 5. 1935).—E. T. Griffith: An Early Motive of Roman Imperialism, pp. 1–15.—G. G. Coulton: Nationalism in the Middle Ages.

The Economic History Review (Vol. 5, April 1935).—T. H. Marshall: Revisions on Economic History: II. The Population of England and Wales from the Industrial Revolution to the World War, pp. 65–79.

The Eugenics Review (Vol. 27, October, 1935).—C. P. Blacker: Laws on Health and Marriage, pp. 191–197.—D. V. Glass: The Berlin Population Congress and Recent Population Movements in Germany, pp. 207–213.—D. Caradog Jones: The Co-ordination of the Records of Births, Marriages and Deaths.

The Journal of the Royal Anthropological Institute (Vol. 65, January to June, 1935).—H. J. T. Bijlmer: The Relation of Blood-Groups to Race and Some Particulars of the South-West Pacific, pp. 123–133.—E. W. Smith: Africa: What Do We Know of It? pp. 83–97.

Man: A Monthly Record of Anthropological Science (Vol. 36, January, 1936).—W. E. Le Gros Clark: Evolutionary Parallelism and Human Phylogeny.—Leonhard Adam: North-West Chinese Art and Its Early Chinese Parallels.

The Political Quarterly, vol. 7 (January–March, 1936). David Thompson: The Implications of Public Morality, pp. 49–60.—G. L. Schwartz: Capitalism on the Dole, pp. 70–83.

Royal Economic Society.—Memorandum No. 56: Enid Charles: The Effect of Present Trends in Fertility and Mortality upon the Future Population of England and Wales and upon Its Age Composition.—*Memorandum No. 55:* Report on Current Economic Conditions.

The Sociological Review (Vol. 27, October 1935).—W. M. Cousins: The Sex-Ratio among Negroes as Illustrated by Certain British Colonies, pp. 423–40.—Werner Falk: Democracy and Capitalism in Max Weber's Sociology, pp. 373–393.—C. G. Field: Bias in Social Study, pp. 394–407.—A. C. Sewter: The Possibilities of a Sociology of Art, pp. 441–453.

SOCIOLOGICAL PERIODICALS OF CZECHOSLOVAKIA

JOSEPH SLABEY ROUCEK
Division of General Education
New York University

The best sociological quarterly of Central and Eastern Europe is the *Sociologickâ Revue*, official journal of the Masaryk Sociological Society of Czechoslovakia, under the editorship of Professor I. A. Bláha of the Masaryk University of Brno, Czechoslovakia (Brno, Neumannova 32). Published since 1930, by the sociological department of the university just mentioned, it has weathered successfully the years of depression, although the subscriptions are falling off lately; on the other hand, the journal has the distinction of being read in all the countries of Europe and in America, thanks to the policy of the editor of having the leading articles summarized in English, French and German.

On the board of editors we find the following specialists in their lines: Docent Dr. Em. Chalupný, for general sociology, sociology of law, state, and economics; Prof. Dr. In. A. Bláha for mesology, population problems, rural sociology, sociology of morals, religion, education, and social pathology; Dr. Frant. Fajfr; Prof. Dr. J. L. Fischer for sociology of culture and theoretical socialism; Senator Frant. Modráček for sociology of socialism, coöperatives, and primitive groups; Dr. Oskar Butter for sociology of newspapers; Docent Dr. Ant. Ráliš for criminology; Deputy Dr. Ant. Štefánek for the sociological problems of Slovakia; and Dr. B. Václavek for the sociology of art. Dr. Ant. Obrdlík, who now studies sociology in this country as a fellow of the Rockefeller Foundation, is the book review editor (together with Dr. Brno Zwicker); thanks to his efforts the leading works in sociology in English and other languages (including Russia, Yugoslav, Bulgarian, etc.) are regularly reviewed.

Quite a number of articles of leading European and American sociologists have already appeared in the *Review*. The international policy of the editor is evidenced by the fact that the following persons are members of the "foreign" editorial board: C. Bouglé (Paris); G. L. Duprat (Geneva); V. Ganev (Sofia); C. W. Hasek (Pennsylvania State College); M. Kosić (Belgrade); M. Mirković (Subotica); G. Richard (Bordeaux); J. S. Roucek (New York University); P. A. Sorokin (Harvard University).

The regular issues of the *Review* are usually divided into the following parts: Part I contains several leading articles, without special regard to their contents; Part II is composed of "documents," such as special reviews of the development of sociology in various countries and special studies of outstanding sociologists and their schools; Part III is subdivided into special sections of sociology—as suggested in connection with the editorial board—each containing book reviews and shorter articles; Part IV has minor news, which cover the meetings of various sociological societies, the deaths of sociologists, and minor items; the final section, Part V, surveys the current publications of sociological interest in minor reviews, often with only several sentences.

If we survey the contents of the last two years, of special value have been analyses "The Sociological and Philosophical Basis of Beneš' Political Theory" (*SR*, 1934, Vol. V, 1–3, pp. 7–15), by Bláha, who relates the practical application of Beneš' political practices to his sociological principles. K. Maiwald, in his "Economic Influences on Families of Public Employees" (*ibid.*, pp. 16–24), ascribes the late marriages of public employees in Czechoslovakia to the maladjustment between the one-sided slow growth of their incomes during earlier years of service and the curve of needs of young married employees, suggesting that the lag should be bridged over by a more effective family-allowance system. Such a solution would also help to mitigate the question of women in public service. Some results of a special sociological study of the various sociological problems in a typical country village of Czechoslovakia appear in A. Obrdlík's "Social Mobility in One of Our Rural Communities" (*ibid.*, pp. 25–52), who finds that, in general, the peasant class is less inclined to mobility than the others and that territorial and the professional mobility is more intensive than religious; and that the social mobility of the community as a whole is now much greater than it was in pre-war days. Cultural mobility is caused by compulsory education. Territorial mobility frequently causes professional mobility; for instance, a peasant going to America changes his vocation and becomes a factory worker. As a rule, however, the two most important forms of social mobility (territorial and professional) influence each other and cause the social shifting of individuals. "The Population Question of Italy from the Sociological Standpoint" (*ibid.*, pp. 53–69) receives a considerable analysis by Roberto Michels. Mykyta J. Šapoval concludes his survey of Ukrainian Sociology (*ibid.*, pp. 70–75), and Mirko M. Kosič discusses the "Social Situation and Spiritual Trends of the Yugoslav Youth" (*ibid.*, pp. 76–80), while a similar problem is treated in Frant. Mirek's "Spiritual Trends of the Contemporary Polish Youth" (*ibid.*, pp. 80–89). The problems of Central European teachers are explored by Jaroslav Šíma in "The Ideal of Monogamy in the Life and Thinking of Future Teachers (*ibid.*, pp. 89–93), and Jan Uher's "Student and Family" (*ibid.*, pp. 93–111). Uher's conclusions resemble those offered by several American sociologists, namely, that the negative relation to the family is in a great many cases produced by the severity and strictness of parents, usually of the father, by the misunderstanding of children by their parents, by differences of viewpoints and character, by certain qualities of the parents, by frequent absences of parents, especially of the father, from home, by economic misery, and in one case by shyness.

The concluding 1934 issue carries Howard Becker's "Sociology in Great Britain" (*SR*, 1934, Vol. V, 4, pp. 251–272), Oskar Butter's careful survey of "The Negation of the Freedom

of Press in Dictatorships" (*ibid.*, pp. 273–95), and Brno Zwicker's "Sociology of Unemployment" (*ibid.*, pp. 296–305), which he concludes in the following issue (*SR*, 1935, VI, 1–2, pp. 34–43). Of special interest are his findings regarding the mental attitudes of the unemployed and radical mentality to politics. Bláha's study of "T. G. Masaryk" (*ibid.*, pp. 7–13) analyzes the Czechoslovakian ex-President's leadership as of rationalistic, educational, and democratic character. Obrdlík's "Sociology of Political Elections" (*ibid.*, pp. 44–54) describes the social processes of Central European elections, as conditioned by socio-psychological, economic, political, social, and cultural factors. Albert Lauterbach's "Problem of the Revolution" (*ibid.*, pp. 68–72) is a Marxian approach. J. Húsek has specialized in "Slav Ethnography" (*ibid.*, pp. 93–97). Quite valuable is the history of the "Masaryk Sociological Society" (*ibid.*, pp. 79–89) by E. Chalupný. J. S. Roucek's "Sociology of the Soldier in War Time" (pp. 164–173) covers especially the socio-psychological aspects of that problem.

The latest issue (*SR*, Vol. VI, 1935, 3–4) contains G. L. Duprat s "Outline of Regional and Urban Sociology" (pp. 251–260). Chalupný's "Fundamental Connections of the Social Structures to the Other Phenomena of Civilization" (*ibid.*, pp. 261–271) is an attempt to relate social structure with five other categories of civilization: education, law, morality, religion, and theory (that is, science and philosophy). E. E. Eubank's "Field and Problems of the Sociology of Religion" (pp. 329–335) covers a field very little explored in America.

While the *Sociological Review* represents mainly the Brno group of sociologists, gathered around Professor Bláha, the Prague group, centralized around Professor Josef Král, has published its own *Sociální Problémy* (Social Problems), since May, 1931 (Prague VII, Šternerkova 1358). Its aim is to "unite all social sciences on one basis" and to unite "social theory with social practice and to aid their mutual coöperation." In contrast to the *Sociological Review* it is more interested in the field of social sciences. Most of the issues are devoted to general articles, then articles surveying a particular field of literature, and only very little space is given to various news, book reviews and notes. Of interest to the sociologist is J. Král's "The Social Sciences and their Classification" (Vol. I, 1, pp. 3–15), Z. Peška's "Study of the Beginnings of Nationalism" (pp. 16–20, 2, 93–100), and especially Jan Mertl's "Ideology in Present-Day Politics" (pp. 21–29, Vol. I, 2, pp. 101–111, and Vol. I, 3, pp. 207–214), which is really a historical survey of the development of the concept of ideologies and various theories regarding their working as social forces. Jar. Korčák s "Contribution to the Theory of Nationality" (Vol. I, 3, pp. 167–189) has created a lot of discussion because of its analysis of the Slavic elements in nationalism. H. O. Ziegler surveys the German literature in the field of "The Crisis of Parliamentary Democracy and Present-Day German Political Science" (Vol. I, 3, pp. 190–201). Quite an extensive survey of foreign literature is provided by Miroslav Šebor's "Sociology and Geography" (Vol. I, 4–5, pp. 347–361). The later issues tend to print less original articles and to contribute more space to various arguments in the field of book reviews and viewpoints and surveys of various fields of literature. Very suggestive is Ant. Paleček's "Peasantry of the Feudal Type and Agrarian Peasantry" (Vol. II, 5–6, pp. 404–414). The social attitudes of the Central European peasants are described by Svatava Pírková in her "The Village under a Cinema Lens" (Vol. III, 2, pp. 65–79). One of the best issues (Vol. III, 3,) was that dealing with "cultural lag" in Europe wherein A. Boháč analyzed "The New Economic and Population Situation" (pp. 139–155), Leopold Šauer "Economic Theories and Economic Reality" (pp. 156–163), Zdeněk Ullrich "The Social Structure of Today" (pp. 164–180), Otakar Machotka "The New Values and the New Structure of Society" (pp. 181–190), Jan Mertl "The Concentration of Political Power" (pp. 191–207), and H. O. Ziegler "Present Society and Old Constitutional Forms" (pp. 208–220). The latest issue (Vol. IV, 4) carries a valuable detailed description of Jan Mertl's "Today's Sociology as Presented in the XIII Congress of the International Sociological Institute" (pp. 245–261).

Quite a number of sociological articles are published in the *Statisticky Obzor* (Statistical Review), now in its sixteenth year, published by the Czechoslovak State Statistical Institute (Prague VII, Bělského 2), wherein we find especially useful the contributions of Docent Dr. Antonín Boháč and Dr. J. Auerhan, docent of Charles University and President of the Institute. *Sociální Revue* (Social Review) is an official publication of the Ministry of Social Welfare (edited by Dr. J. Kotek, Prague II, Palackého nám. 375), also in its sixteenth year. The acute problem of minorities is explored theoretically as well as practically and descriptively, in the best quarterly of its kind of Central Europe, *Národnostni Obzor* (The Review of Nationalistic Problems), edited by a well-known authority in this field, Dr. Emil Sobota (Prague XIX, Bubeneč, Terronská 10) and published by the Society for the Study of Nationalistic Questions, now in its sixth year.